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THE PSYCHOLOGICAL REVIEW

TOTAL PATTERN OR LOCAL REFLEXES?[1]

BY ZING YANG KUO[2]

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INTRODUCTION

Development of behavior, until very recent years, has been supposed to proceed from simple reflexes to complex movements. While there was little actual observation on the somatic activity of embryos and fetuses, it was assumed that prenatal and even new-born behavior started with a group of reflexes which were later combined to form more complex movements as development proceeded. Even adult learning has been interpreted as a process of chaining simple reflexes into compound activities which both neurologists and psychologists have agreed to call 'habits.' The whole framework of the Watsonian behaviorism is also built upon the reflex concept.

Although during the last fifteen years or so the concept of reflex has been severely criticized by a small group of speculative psychologists of various brands, whose argument was based on ideological reasoning rather than on laboratory findings, it remained unchallenged in neurology until the theoretical interpretations of Coghill's embryological work on the development of amblystoma were published. Coghill, as we now all know, claims that behavior in amblystoma starts with total movements, or 'total pattern,' as he prefers to call it, and that 'local pattern,' or local reflexes appear only in the later stages of development and are derived or 'individuated' from the total pattern. Such a sequence of

[1] Based on a lecture delivered at Brown University, December, 1937.

[2] This paper was prepared while the author was at Yale University.

development, which reverses the developmental order implied in the traditional reflex theory, is applicable not only to amblystoma but also to other vertebrates. He even goes so far as to apply this theory of development to such speculative psychological problems as 'Gestalt' (13), 'instinct' (14), 'mental conflict' (17), and 'motivation' (20). But as it is my intention to confine the discussion to embryological findings, we shall not touch upon any psychological subject of such a speculative nature.

Coghill might have revolutionized neurology if his views were confirmed by the results of other workers on various other vertebrates. While Angulo, who bases his conclusions on his rather extensive work on the behavior of rat fetuses, and Tuge (41), who has done some work on pigeon embryos, have unreservedly supported Coghill's contention; and while Hooker (23), who has been working on human fetuses, seems also to favor, with reservations, the Coghillian sequence of development, Windle and his associates, working on the fetuses of the cat, rat, and sheep, have repeatedly challenged the adequacy of Coghill's interpretation and have proposed a new theory, which, as a matter of fact, amounts merely to the restoration of the traditional reflex theory in its simplest form (44). Very recently, Windle's view was shared by Joseph Barcroft and D. H. Barron (6). The work of Carmichael on the fetuses of guinea pigs (10, 12) also tends to throw some doubt on Coghill's generalization. But Carmichael seems to be very careful not to revert to the extreme form of the reflex theory which has been sponsored by Windle and Barcroft. Thus, at present we have two opposite views regarding the sequence of behavioral development in the embryo with most of the workers in the field divided into two distinct, opposing camps. Both claim to have based their conclusions on anatomical and physiological findings from their studies on various vertebrate embryos and fetuses, but, curiously enough, neither side has questioned the accuracy or reliability of observations made by the other. It is my purpose in this paper to examine very briefly but critically the anatomical and physiological data from both sides of the

controversy and their theoretical interpretations, and then to present a brief resumé of my own observations on the chick embryo, which have some bearing on this controversy as well as on my own theoretical stand in the matter.

Anatomical Considerations

In this section I shall give a brief critical review of the works by Coghill, Angulo and Windle. Only two of Coghill's generalizations will be considered here: (1) In the amblystoma a stage in which "the animal first responds to light touch on the skin" is presaged by a myogenic, or non-motile stage, in which the mucles can be excited to contraction by direct stimulation, as by the stab of a sharp needle, by mechanical impact or by electricity, but cannot be excited by light touch on the skin. (2) In their early development, the fore and hind limbs and the gills move only when the trunk moves and it is only in a later stage of development that these organs are able to move independently of the trunk. Coghill then undertakes to give anatomical accounts of the development of the nervous system to explain (*a*) the difference between the non-motile stage and its succeeding stage which, according to him, is characterized by nervous excitation; (*b*) why the limbs and gills at first cannot move independently except under the domination of the trunk; and (*c*) why such organs are later emancipated or 'individuated' from the 'totalitarian dictatorship,' as it were, of the trunk and become capable of independent reactions.

The anatomical difference between the non-motile stage and the earliest nervous excitation stage, according to Coghill, is this: In the non-motile stage there are "both sensory and motor nerves in contact with their respective organs, the sensory field on the one hand and the muscles on the other, but the anatomical relations between these systems are such that an excitation cannot pass from the sensory to the motor mechanism" (13, pp. 11–12). "With the ability to respond to tactile or chemical stimulation of the skin there appears a third series of cells. They bridge the gap between the sensory system of one side and the motor system of the other. Their

bodies lie in the floor plate of the medulla oblongata and upper part of the spinal cord" (13, pp. 12–13). Personally I do not think that any one can raise any reasonable objection on anatomical grounds to such an explanation of Coghill's. However, from the physiological standpoint two questions remain to be decided before Coghill's anatomical explanation can be finally accepted: (1) When do the sensory and motor systems in the amblystoma begin to function? Does their function begin only at the time when the embryo first responds to light touch on the skin? Or, are either or both of the sensory and motor systems already in functioning condition before response to light touch can be elicited? This leads to the second question. (2) Is the ability to respond only to the stab of a sharp needle or mechanical impact, or electricity, but not to light touch on the skin by human hair, a sure evidence of the absence of nervous control, or is it merely an indication that in this so-called non-motile stage the threshold for nervous excitation is so high that it requires a much stronger stimulus than a single human hair? In other words, is the behavior in the non-motile stage truly myogenic, as Coghill has claimed it to be, or neurogenic? All these questions can only be answered by more refined and quantitative physiological methods. Until further data from such methods are available, the conclusions of Coghill based on purely anatomical data and on rather crude methods of stimulation by such agencies as sharp needle, human hair, mechanical impact and electricity, must remain doubtful. In this connection, it is worth while to remember the recent remarks by Youngstrom (47) that in the anuran embryo it is doubtful that the response to mechanical stimulus does not involve any neural function and that it is practically impossible to stimulate the leg mechanically without striking over distributing branches of a cutaneous nerve.

Coghill's anatomical explanation of the dominance of the trunk over the arm is based on the histological observation that in this animal the motor nerves which innervate the trunk muscles send out branches to invade the territory of the limb. In the case of the subjugation of the gills to the trunk

the anatomical relation is somewhat different. The nerves that innervate the gills are not the ones that branch out from those which go into and innervate the trunk muscles. However, "the motor cells of the group which are to innervate the muscle of the gill are situated in the most dorsal part of the motor division of the nervous system. Large dendrites from these cells invade all sources of afferent impulses through the sensory system, the ascending trigeminal tract and the lateral line tract. They reach also into the zone of synapse between the commissural cells of the floor plate and the motor neurones that innervate the trunk muscles. The muscles of the external gills are therefore subjected to excitation not only by those afferent impulses which are on the way to the motor mechanism of the trunk, but also by those efferent impulses of the motor path which are on the way directly to the muscles of the trunk" (13, pp. 31–32).

The results of anatomical observations such as these are extremely valuable in showing actual neural connections between different parts of the body and in giving a structural explanation of the fact that the gills and the limbs may and do move with the trunk. But when it is insisted that such neural connections also account for the fact that in the early stages of development the limbs and gills are necessarily under the domination of the trunk, that they move only when the trunk moves, and that it is by the process of 'individuation' that they later acquire the ability to respond to stimulus independently of the trunk, I, at once, begin to question the validity of such a conclusion. Through the nervous system all parts of the body are interconnected in some way or other and it is natural that when circumstances are favorable, nervous impulses which excite the muscles of one organ may also excite some other organs in the farthermost distance. When the tail of a chick embryo is stimulated with strong electricity, it responds and at the same time the eyes open and the beak claps. Of course, it is highly possible to demonstrate anatomically that the nerves which innervate the eyes and the beak are, in some way, indirectly connected through some parts of the central nervous system with the

nerves which innervate the tail. But such an anatomical demonstration merely proves that it is structurally possible for two distant parts of the organism to be involved in a response to stimulation; it does not necessarily prove that the eyes and the beak of the chick can move only when the tail moves, for eye and beak movements do very frequently occur independently of the tail. It requires more than a mere anatomical demonstration of neural connections between parts in order to prove the validity of Coghill's principle of development; it is necessary, first of all, to prove that, at least in the early stages of development, local movements of any part of the organism are totally absent under any circumstance, since the nervous system is integrated from the start, to use Coghill's expression; and, secondly, it must be explained either anatomically or physiologically why in a later stage of development local movements may and do occur in spite of the fact that the nervous system has already been integrated for some time. Let us see if Coghill has fulfilled these two requirements.

Coghill has, of course, already shown that under normal conditions in the amblystoma the limbs and the gills at first move only with the trunk. But even in such primitive vertebrate embryos upon which Coghill bases his generalization, local limb reflexes can be evoked before the time which has been designated by him for their first appearance. This is demonstrated by Coghill himself. To quote from his observation: "If a larva of this species [*Amblystoma tigrinum*], soon after the fore limbs move with trunk movement but before limb reflexes can be evoked with the animal in its normal upright position, is placed upon its back under conditions which twist the tail and trunk slightly upon the longitudinal axis, the fore limbs assume a definite posture. The limb on the side thrust deeper towards the bottom of the receptacle is then abducted and elevated while the other limb is adducted and relatively depressed. While in this posture, the extended limb will respond to a touch upon its surface by a further quick abduction and elevation" (13, p. 21).

This observation clearly demonstrates that the time for

the first appearance of local reflexes of the fore limbs in the amblystoma is not definitely fixed but rather depends upon the conditions under which the behavior of the embryo is observed at the moment. In upright position it moves with the trunk, in supine position it moves independently. The interesting point for us to note here is that in the amblystoma upon whose behavior Coghill's principle of development of behavior is based, and at the stage of development at which, according to his principle and on the basis of his anatomical findings, there should be nothing but total movements, local reflexes of the fore limb do occur when the position of the animal is changed, and they occur in spite of the fact that the structure of the nervous system, which, according to Coghill, is responsible for the fact that the limbs can move only with the trunk, remains relatively unchanged.

Youngstrom has most recently reported (**47**) that in anuran embryo (*Psendacris triseriata*) in the stage of spontaneous movements of the legs, in which the legs do not make any response to mechanical stimulus or light touch and during which the behavior is characterized by massive movements, a discrete spontaneous movement of the limb may be occasionally observed while the animal is apparently at rest and not stimulated. In my own study of several native species of *Rana* in Nanking (**35**), it was found that about 8 per cent of the first spontaneous limb movements, 10 per cent of the first leg responses to mechanical stimulus and 11 per cent of the first responses to light touch belong to the category of the so-called independent reflexes. As we shall see more clearly in another section, observations such as these lend no support either to the reflex theory as now championed by Windle, or to Coghill's principle of total pattern, from which local patterns are supposed to be 'individuated.' My point is that in any given stage of development whether the limb or any other organ will move independently or in conjunction with other organs depends upon a number of physiological factors. It is not primarily determined by the structure of the nervous system, nor does it involve the question of time order, that is to say, which of the so-called total pattern or

the so-called local reflex appears first in development and which is derived from the other.

Coghill's anatomical explanation of individuation or appearance of local reflexes is rather vague and uncertain. In 1929 he wrote: "It appears, therefore, that the individuation of a partial pattern or local reflex within the total pattern is anticipated in the central nervous system by the growth of a nervous organization with specific reference to that partial pattern long before the latter makes its appearance in behavior. The details of this process are still to be determined; but its general nature is obvious" (13, p. 91). Evidently he refers to the growth and organization of the brain and the spinal cord in general, but at just what point of growth, by what part of the organization of the central nervous system, and in what way it is made possible for local reflexes to appear in behavior seems to me a mystery.

However, in a later publication (15) Coghill assumes that there are two motor systems: (1) 'the mechanism of total pattern action or primary motor system' and (2) 'the mechanism of local reflex, or secondary motor system.' He finds that during the total movement stage, the only motor fibers perceptible in the limb are the branches of those which innervate the muscle of the trunk. He designated these branches of motor fibers as primary motor system and considers them as the mechanism of the total pattern of action. "As reflexes appear, motor fibers from another region in the spinal cord join the primary motor fibers in the nerve roots." He presumes that these new fibers are the final common path for local reflexes and hence constitute the mechanism of local reflexes or secondary motor system. He further assumes that local reflexes result from the inhibition of the mechanism of the total pattern and excitation of mechanism of local reflex. Until there are available objective and actual physiological data which will prove that the so-called secondary motor system is really the mechanism of local reflexes and will show how its excitation will at the same time produce an inhibition on the so-called primary motor system, I shall remain skeptical about Coghill's interpretation. In still later

publications (18, 21), Coghill considers the proliferation of cells in the tenth to twentieth segments of the spinal cord as well as the growth of spinal ganglia as factors in the individuation of reflexes of the hind limb. But just how proliferation of cells and growth of spinal ganglia will result in the individuation of the reflex of the leg is not explained.

Angulo finds that in the development of the nervous system in the albino rat the motor system develops and is totally integrated before the sensory system appears. This accounts for the fact that in the rat total movements appear before local reflex. Our criticism of Coghill's anatomical explanations of the domination of the trunk over the gills and limbs in amblystoma may be applied to Angulo's interpretations of the development of the nervous system in the rat; for what Angulo has found is an integrating mechanism which may explain why the limb often moves with the trunk but which does not exclude the possibility that the limb in this stage of development may also move relatively independently of the trunk. In fact Windle has reported that the earliest limb responses of the fetal rat are isolated reflexes. Angulo has objected to Windle's interpretation of his findings. He believes that what Windle has observed as true reflexes are really myogenic responses. We shall see in the next section whether the criteria used by Windle and Angulo to distinguish between myogenic responses and true reflexes are valid.

In another place Angulo states that "When the receptive mechanism first develops the axones of the sensory system run directly cephalad to these nuclei (nuclei of the fasciculus gracilis and cuneatus) without giving off collaterals. Thus, all the stimuli received by the animal are carried cephalad to these associative nuclei, whence they find their way to the effector mechanism, and bring about total movements. Later collaterals grow from the axones of the ascending path into the local motor centers and make a local reaction with the expression of the local sign possible" (22, p. 269). Here Angulo seems to have made a strong case on anatomical grounds in favor of the total pattern principle. It is, however, necessary to show that the early responses to exteroge-

nous stimuli appear before there are any collaterals from the ascending path into the local motor centers and should be nothing but massive movements and also that the definite date for the first appearance of the local responses corresponds with the appearance of a definite number of such collaterals. Unfortunately Angulo has not worked out such requirements satisfactorily. The exact date of the first response to exterogenous stimulation, the part of the organism which makes such a response (for instance, the snout region or the limb), the question as to whether such a response is massive or localized, and whether there are any localized spontaneous movements of a nervous nature before the first response to external stimulus appears—all such matters must be definitely settled beyond any dispute before anatomical interpretation of such a nature can be evaluated.

Windle and his associates have studied several mammalian forms such as the cat, the rat and the sheep. They have also correlated their behavioral findings with the development of the nervous system in fetal kittens and rats. The results of their anatomical and behavioral studies on these several forms seem, according to Windle, to be consistently in favor of the reflex theory in development. I shall quote only one statement from Windle in regard to the correlation between the appearance of reflexes and the development of the nervous system in the rat. "The increase in number and length of sensory collateral fibers in branchial spinal segments is correlated with the appearance of certain induced behavioral responses of the embryo which have the characteristics of simple, spinal type reflexes. Enumeration in alternate segments unilaterally in five motile and five non-motile embryos of comparable age (about 377 hours) indicates a marked difference. On the average, there is an increase of 85 per cent in number of collaterals in specimens showing the first reflexes over those from which the only elicitable response was muscle contraction due to direct stimulation" (44, pp. 202–203).

In evaluating Windle's anatomical work one must bear in mind that we have no information regarding the beginning

of the function of reflex arcs. Numerical difference in the sensory collateral fibers cannot be used as a criterion for the beginning of such a function, since anatomical facts do not always give a true physiological picture. Furthermore, the appearance of reflex arcs does not necessarily preclude the possibility of the appearance of total movements in the same stage of development or even earlier. As already referred to, Angulo has claimed that the motor system in the rat is integrated in advance of the appearance of the sensory system. In other words, the presence of reflex arcs in the stage of development under discussion may prove that by the nature of the nervous structure local movement in response to local stimulation is made possible, but it does not exclude the possibility that at this very same moment of development a local stimulation may also evoke a widespread response instead of a strictly localized one, since reflex arcs even at their earliest appearance are not structurally isolated or detached from other parts of the nervous system. Indeed, anatomical demonstration of reflex arcs does not necessarily prove the reflex theory any more than does anatomical demonstration of the nervous connection between the trunk and the limb necessarily prove the total pattern theory. I mean to point out that as far as the anatomical evidences reported by Coghill and Angulo on the one hand, and by Windle and his associates on the other, are concerned, there is nothing which will support the view that the so-called total pattern action and local reflexes are mutually exclusive and that either one should appear after and is derived from the other. And, as we shall see more clearly later on, this view is erroneous physiologically for we have gathered sufficient facts to show that the total movements and local reflexes may be present in any stage of development.

Concluding Remarks

From the above discussion it must be clear that the anatomical data presented by Coghill, Angulo and Windle cannot be considered as adequate and unequivocal evidences either for the total pattern theory or for the reflex theory. It seems

to me that the whole problem is a physiological one; neuro-anatomy has not contributed much toward the solution of the problem. We must bear in mind the fact that physiological interpretations on the basis of neural structure is sometimes a dangerous thing as neuro-anatomy may not give the true picture of physiology. Furthermore, unless the beginning and development of the function of the parts of the nervous system concerned are definitely known, structural correlations with behavioral development may not be dependable. It is not my intention here to minimize the importance of anatomical study of the development of the nervous system, but rather, to caution against any attempt to base physiological interpretations on anatomical data. The results of anatomical studies are valuable in so far as they are employed merely as a check on physiological findings. But when one begins to use anatomical facts as major data and interpret them in terms of function without sufficient information from direct physiological experiments, one is actually engaged in a speculative neurology which has had the outstanding record of being an indispensable handmaiden of the armchair psychologist. It is rather unfortunate that in this new and promising field of physiological embryology, certain workers, notably Coghill, should go beyond their laboratory findings and allow themselves to be engaged in philosophical speculation on psychological problems.

Let us now turn to the physiological side of the controversy.

Physiological Considerations

The defects of recent physiological studies

While in recent years numerous publications on the physiological or behavioral development of various vertebrates ranging from fishes and amphibians to birds and mammals and human fetuses have appeared, few have been free from the following shortcomings:

1. So far, we have been testing only the gross bodily movements of embryos and fetuses as a crude indication of the functioning of the nervous system. Very little direct

physiological experiment has been done to find out the actual beginning and development of function of different parts of the nervous system which innervate specific organs of the body. The physiology of the nerve and muscular contraction in the embryo is still unknown. In other words, in embryological studies we are still on the gross behavior level, we have not as yet come down to the actual physiological level.

2. The methods used for giving stimulus and for recording embryonic movements are still very crude. Besides the motion-picture camera we have no adequate instrument for recording embryonic movements; we still depend very much upon the keenness of our visual observations, the correctness and completeness of our immediate memory and pencil-notes for making records of our embryonic studies. Every stimulus for eliciting embryonic responses is given by hand without adequate quantitative measurement. Thus, it has been, so far, almost impossible to state our data on the embryonic behavior in quantitative terms.

3. In the case of mammalian fetuses, the following defects and limitations are apparent:

A. After operation, the fetus is exposed to an abnormal environment. It can live for only a short period of time. Thus, the movements observed under such conditions are not normal, but depend on (*a*) the health condition of the fetus at the moment, (*b*) the blood condition of the mother, (*c*) the length of time after the fetus has been exposed, (*d*) the temperature of the room, (*e*) the environmental conditions to which the fetus has been exposed, and (*f*) in a great many cases behavior observation is made when the embryo is already in the dying state. Thus, what has been observed under such conditions may not be the same as what might happen when the mother is not operated upon and the fetal membranes are kept intact.

B. Unless the operative techniques employed by Nicholas (**37**) and Hooker and Nicholas (**25**, **26**) and Bors (**8**) are carried out far enough, or unless Nicholas's (**40**) method to cultivate mammalian embryos outside of the maternal body has been advanced far enough for physiological studies, it is

impossible to apply the methods of control experiments to our studies of fetal movements or to make continuous observation on the same individual from stage to stage.

C. In most cases the number of fetuses employed for behavioral studies is not large enough to make statistical treatment possible. Generalization without sufficient number of cases is not a sound scientific procedure.

D. Thus far, the stages of physiological or behavioral development of the mammalian fetus have not been worked out and standardized as have been done in amblystoma and in the chick embryo (33).

Problems stated

In evaluating the views of Coghill and Angulo and of Windle and Barcroft from the physiological point of view, we must critically examine the following problems:

1. What criteria may we employ to distinguish between myogenic and neurogenic responses in the embryo?

2. On what basis may we decide whether or not a given embryonic response is a true reflex?

3. What constitutes a total pattern or massive movement, and what a local pattern or a local reflex?

4. What are the factors which determine the type of movement the embryo will make in response to a given stimulus in a given stage of development?

Unless these problems are clarified, any discussion concerning the Coghill-Angulo vs. Windle-Barcroft controversy will add to the confusion already created by the controversialists rather than ameliorate the situation. In the following discussion it is my purpose to point out that (1) the criteria used by Coghill, Angulo and Windle for determining myogenic response and true reflexes are not physiologically sound, that (2) their classification of embryonic movements into two main categories, total pattern vs. local reflex, is rather arbitrary, that (3) how many parts of the body will be involved in a response made by the embryo to a given stimulus is determined by a number of factors, physiological as well as environmental, and that (4) development, therefore, does not

follow the Coghillian sequence, nor does it proceed in accordance with the reflex theory.

Myogenic vs. neurogenic responses and the criteria of reflex

Most of the workers in this field agree that there is a distinction between myogenic and neurogenic responses in embryonic motility. However, there is no accepted criterion for such a distinction. Windle seems to base the distinction entirely on the response side; for him, both electrical and mechanical stimuli may evoke either myogenic or neurogenic reactions, depending on the nature of the reaction rather than on the type of stimulus. On the other hand, Coghill and Angulo seem to have consistently insisted that in early stages of development responses to electrical and mechanical stimuli are myogenic while only those to light touch (hair) are genuinely neurogenic. However, I may say at once that the mere fact that embryo responds only to electrical and mechanical stimuli but not to light touch does not necessarily warrant the conclusion that the response is myogenic. As has been pointed out, it is highly possible that in the very young embryo the threshold for a nervous response may be very high so that a wide area of stimulation and strong stimuli, such as electricity or stabbing of a needle are required to evoke a response. Unless the threshold and the time-relation of embryonic responses to different types of stimulus are accurately measured from stage to stage, we seem to have no way of telling whether the failure to respond to light touch is due to the lack of neural development or to the fact that the stimulus is below the threshold. As Youngstrom (**47**) has pointed out in connection with his study of the behavior of anuran embryos, although the nature of the response to deep pressure is not certainly known, it is probably not due to the direct stimulation of muscles.

The results of the investigation by Coghill (**22**), Angulo (**3**), and Tuge (**41**) to demonstrate the myogenic nature of certain embryonic responses by means of curarization are open to many objections. There is no detailed report of their experiments. The drug was not used systematically. No attempt was made to determine concentration and dosage of the drug

and age differences in susceptibility to curarization. Nor was there any control experiment. The manner of administering the drug was also not stated. The number of embryos employed was not sufficient to warrant any conclusion from statistical standpoint. The mere fact that certain embryonic movements fail to disappear after curarization is not a sure evidence of their myogenic nature unless the experiment is carried out systematically.

On the other hand, when curarization experiments were conducted on the embryonic chick more thoroughly and more systematically, the result was entirely different. I have elsewhere (34) reported in some detail that in any stage of the development of the chick both spontaneous movements and reactions to external stimuli can be either partially or totally abolished by curarization, depending on the dosage of the drug. When the drug is sufficiently strong to abolish certain spontaneous movements, these movements are no longer elicitable by external stimuli until recovery sets in, and on the other hand, when the dosage of the drug is small and can abolish only some of the movements while the other movements remain relatively unaffected, the unabolished movements are still responsive to electrical stimulation. This clearly demonstrates that as far as the chick embryo is concerned there is no myogenic spontaneous movement or myogenic response to stimulus at any stage of development, since every somatic movement may be paralyzed by curare if the dosage is sufficiently strong.

While it cannot be denied that skeletal muscles are capable of automatic contraction of some sort, there is neither factual nor theoretical basis for the view that in embryonic development nervous response is necessarily preceded by a stage of myogenic movement as Coghill and Angulo have insisted, and as is also implied in various reports by Windle and Barcroft and their associates. There is no anatomical evidence that at the time when somatic movements, whether spontaneous or in response to electrical or mechanical stimulations, are first observed, the skeletal musculature is entirely devoid of nervous supply, as is the case in the development of cardiac

contraction in the chick. Nor has there been any physiological demonstration that the skeletal muscle becomes functional earlier than the nervous tissue which supplies it. As has been pointed out before, results from stimulation by such crude and non-quantified agencies as mechanical pressure, electrical shock and human hair cannot be accepted as conclusive evidence for either the myogenic or the neurogenic point of view. On the other hand my own experiments with curarization definitely point to the fact that the peripheral system, at least the motor element, is already in the functioning condition from the very beginning of observable somatic movement, either spontaneous or elicited by any method of stimulation.

Windle and Baxter (44, p. 200) enumerate the following nine points as criteria for the distinction between myogenic responses and true reflexes:

Myogenic phenomena	Reflexes
1. Are relatively slow contractions	1. Are relatively quicker contractions
2. Tend to be maximal	2. Are not maximal contractions
3. Have no visible latent period	3. Have noticeable latent period
4. Appear without summation	4. Sometimes require summation
5. Are maintained contractions	5. Quick return. No tetanus
6. No fatigue period follows	6. Long fatigue period follows
7. Localized contraction; can be molded by position of stimulus	7. Are stereotyped movements
8. Are relatively unaffected by health of embryo and parent	8. Are very dependent upon good health of embryo and parent
9. Structural reflex arcs are incomplete when they first appear	9. Reflex arcs are present when they first appear

Most of these nine points are quantitative. But unfortunately, so far we have not had any adequate instrument to record and measure accurately embryonic movements. Windle and his associates, in making the distinction between myogenic responses and reflexes, merely based it on rough estimates without the aid of exact recording instruments and accurate device for measuring time. Moreover, we have very little information concerning the physiology of neuromuscular contraction in the embryo. Unless such important phases of the embryonic neuro-muscular physiology as threshold, time-relation between stimulus and response, summation, latency, refractory period, fatigue, relaxation, maximal contraction etc., can be accurately recorded by instruments, their quantification stated in exact terms, and age differences during embryonic and fetal development determined, comparison between myogenic responses and true reflexes on the basis of such phenomena seems to be of little value (*cf.* Hooker, **23**).

As has been previously stated, unless the beginning of the function of a reflex arc is determined, the increase in the number of collateral branches of primary afferent neurons cannot serve as an adequate evidence for the appearance of a reflex. The difference in the number of sensory collaterals between those rat embryos whose reactions are considered by Windle and Baxter as myogenic and those which are capable of reflex response is merely a quantitative one. Anatomically there is no reason why a response should be regarded as non-nervous just because at the time of its appearance the number of sensory collaterals is considerably smaller. Until the minimal number of collaterals necessary for the first appearance of a reflex is definitely determined for different vertebrates and until other criteria for the distinction between myogenic and neurogenic responses are established, numerical differences in the collaterals tell us nothing as to whether or not a given response is of neurogenic origin. I am not arguing, as does Angulo, that what Windle has called reflexes are myogenic in nature. On the contrary, I am inclined to think that even those responses which Windle himself has

classified as myogenic may be of nervous nature. Indeed, pending further physiological information by more refined and accurate methods, the myogenic point of view which assumes that in ontogenic development somatic movements of a nervous nature are antedated by a stage of non-nervous muscular contractions should be looked upon with a great deal of suspicion.

The distinction between total pattern and local reflex

It is customary in developmental studies to classify behavior in two major categories, 'total pattern' and 'local reflex,' and any movement observed in the embryo must belong to one class or the other. The question I wish to bring up is: What constitutes a 'total pattern' and what a 'local reflex'? Is there any really independent movement of any part? Is there any embryonic movement which involves the whole body? In the so-called 'total movements' is it necessary that either the trunk or the head be active and play a dominant part? I have observed literally thousands of chick embryos and my answer to these questions is negative.

In the first place, there is no real independent reflex of any kind. The limb may be seen to move independently of other parts, but upon closer observation it can always be detected that slight movements of some other parts are actively involved. An independent or local reflex is more apparent than real; it is almost invariably accompanied by movements of neighboring regions which are often so slight that they escape observation. On the other hand, in the so-called 'total pattern' or massive or generalized movements, I have not seen a single instance in which the whole body is actually involved; there are always certain parts of the body which remain inactive. Embryonic movements vary from time to time in the degree of complexity, in the number of regions or organs involved in the response. Between the one which may be regarded as almost a total movement and the one which may approach the ideal type of local reflex there are numerous other movements with varying degrees of com-

plexity. There is always a quantitative difference between two movements with regard to the number of parts of the body involved. But there is no qualitative or absolute difference which may separate embryonic behavior into two classes. 'Total pattern' and 'local reflex' as two distinct categories in embryonic motility must be viewed as a scientific fiction. The real question is not whether a given movement falls into one class or the other but rather "what and how many parts of the body are actively involved in a given movement."

In the second place, in the chick there are periods of development in which the so-called massive or generalized movements often take place without the active participation of the head and the trunk. During the second week of incubation the legs become most active and prominent in movement while the magnitude and frequency of trunk movement is gradually reduced to a minimum. So in the so-called 'massive movement' the legs generally take the leading part while the trunk participates only slightly or does not move at all. Here we have a case of the so-called 'total pattern' which does not strictly fit in with Coghill's theory which implies the domination of the trunk over other parts. Furthermore, in the chick the number of parts actively engaged in a so-called 'generalized movement' varies from time to time. The actual number involved each time and what parts will participate depend on a number of factors which will be discussed shortly. The movements of the participating parts may not start simultaneously, but are often seen to start in rapid succession. The question as to which part starts first depends upon such factors as the point of stimulation, the position of the embryo, the direction and manner of amnion contraction, etc. I am here pointing out the rather extreme complexity and variability of embryonic movements which do not seem to follow any such oversimplified theoretical schemes as have been sponsored by Coghill and Angulo and by Windle and Barcroft.

Factors Determining the Type and Nature of Embryonic Motility

I shall here give a brief resumé of the experimental evidence regarding the so-called independent or discrete reflexes. Whether a given movement will be relatively restricted to a local region (the so-called 'local reflex') or will spread over other regions depends upon the following factors:

1. *Developmental stages.*—The so-called local reflexes, that is to say, movements which are relatively restricted to a local region or to a single organ (such as a limb, the tail, one eye and the beak, etc.) without apparent or conspicuous participation of other regions or other organs, may appear in any stage of development. Neither the notion that local movements can be observed only in later stages, nor that local movements are the earliest somatic activity of the embryo, has found any confirmation in my own observation. In several species of frog embryos which I have studied about 8 per cent of the first spontaneous limb movements, 10 per cent of the first limb responses to mechanical stimulation, and 11 per cent of the first limb reactions to light touch were local movements. Youngstrom (47) also reports that discrete spontaneous limb movements may be occasionally observed in anuran embryos before the limb is capable of reacting to mechanical or tactile stimuli. In the chick a small percentage of the first limb movements, both spontaneous and in response to external stimulation, belongs to the class of the so-called local reflex. Carmichael has recently pointed out that in the guinea pig local movements may be present in the early stages of development, but are often masked by more massive movements (12). However, the percentage of local movements in the early stages in all these studies is far from being sufficient to substantiate Windle's claim that the development of embryonic motility begins as local reflex.

In the chick the percentage of spontaneous local movements varies from stage to stage. I have divided physiological development of the chick into ten stages (33). Movements involving different parts of the body in different

stages have been systematically studied under various conditions (35).

Several facts found in this investigation are rather significant in connection with our discussion.

(1) There is a small percentage of leg movements which are relatively independent or localized when they first appear.

(2) From stage to stage there is a steady increase in both number and percentage of independent leg movements.

(3) In the stages during which the embryo becomes quiet and somatic activities are reduced (stages 7, 8 and 9) the number and percentage of local movements are tremendously increased.

(4) During hatching (stage 10) when there is a sudden burst of violent movements on the part of the embryo, the number and percentage of independent movements drop down to a very much lower level than that in the two preceding stages. In general, it may be concluded that in spontaneous activity local movement appears in every stage of development, but in the earlier stages its appearance is so rare that it is generally overshadowed and obscured by other movements in which many parts of the body are actively engaged, and that its frequency increases as development progresses. It is, however, conditioned by the general somatic activity of the embryo during each stage, that is to say, the more active the embryo is, the less local reflexes and vice versa.

2. *Health conditions of the embryo.*—Windle, Bishop, and Steele (45) have reported that anoxamia tends to increase the so-called total movements in fetal kittens whereas with a large supply of oxygen local reflexes predominate. My own observation of the chick is somewhat different. In the chick, when hemorrhage, cooling, or other injury occurs, or when the embryo is weak or dying, or when artificial anoxamia is induced, movements both spontaneous and in response to stimulation become weaker and fewer but more localized than in embryos of the same age in normal health. However, the fact that the so-called local reflexes are conditioned by the health condition of the embryo is obvious both in the bird and in mammals.

3. *Position of the embryo.*—In amphibia, Coghill has stated that when the embryo is placed on its back, local movements of the limb can be elicited earlier than normal. In the chick embryo, independent limb movements are observed much more frequently when it lies on its side, or in prone position or when the head is buried in the yolk, than when it is on its back. Unfortunately the workers in mammalian fetuses have not reported any significant difference in movement which may be attributed to the effect of bodily position.

4. *The effect of environmental conditions.*—In the chick the environmental conditions have a decisive influence on local movements. During stage 6, the stage of hyperactivity, local movements are rather rare. The almost ceaseless and powerful contractions of the amnion often stir the animal into violent movements. However, when the amnion ceases to contract for a while or when the magnitude of amnion contraction is reduced, local movements become relatively more frequent. Or if the egg is so turned that the yolk and the yolk sac crowds the embryo in such a way as to interfere with the expansion of the wave of the amnion, the movements of the limb will be more likely to be localized than when the amnion is not 'compressed' by the yolk. During the last week of incubation, the size of the embryo is greatly increased and the liquid contents of the egg are reduced so that the embryo lives in a very congested environment closely surrounded by the yolk and the membranes within the shell, while the head and the neck are bent and the legs are folded down close to the breast. Under such conditions, movements are greatly reduced in frequency and magnitude and localized movements are much more frequently observed than in any previous stage, and may sometimes outnumber other types of movements. In stage 6, if the amnion is removed, localized movements will increase.

5. *The effects of drugs.*—Curarization tends to increase the frequency of local movements both spontaneous and in response to external stimulation so long as the dose is not large enough to cause total paralysis of movements (34). Furthermore, in the undrugged 5-day embryo, we have not

thus far been able to observe any independent limb movements in response to faradization. But when embryos of the same age were curarized and their motility was partially paralyzed, I was able to elicit such independent responses either by electricity or by human hair.

6. *Intensity of stimuli.*—In spite of the fact that the Harvard inductorium is a very crude instrument for quantitative purpose, the result of our study seems clearly to indicate that within a certain limit the weaker the electrical stimulus, the more likely the response of the chick embryo is to be localized. While more refined quantitative methods should be devised in order to secure more exact information on this point, the fact that a stimulus of greater intensity tends to involve more parts of the body in the response seems to be well-established. It should be noted in this connection that recent work of Carmichael (**12**) on fetal guinea pigs seems also to imply that the intensity of stimulus (human hair) is rather an important factor in determining whether the response induced by such a stimulus will be localized or will spread.

7. *Time interval between stimuli*—In the embryonic chick, if the first stimulus evokes massive response, the second stimulus of the same intensity, when it is given within an interval of less than 20 seconds, may fail to call out any response, or may merely call out a local one. In fact, if the second stimulus is given in too short an interval the possibility of inducing a localized reaction is in many cases greater than that of evoking a more massive one.

8. *The number of repetitions of stimulus.*—Statistical results presented in another paper (**35**) tend to show that if the same stimulus is repeated with a fixed time interval more than 10 times, the percentage of local limb movements is considerably increased.

9. *The strength and magnitude of the initial movement.*—In a great many instances the parts which are actively engaged in a so-called massive movement do not start to move synchronically, but rather follow rapidly the initial movement of a certain part. The results of our observation show that the strength and magnitude of the initial movement is also a

determiner of local movements. In general, if the initial movement is weak or circumscribed, the chances for it to be localized are much greater than when it is strong or extensive. This suggests that the initial movement may serve as an effective stimulus for movements of other parts.

10. *The relation between active body parts and massive movements.*—In the chick during the first week of incubation the trunk is most active and prominent in movement. During this period most of the massive movements are generally initiated by the trunk. On the other hand, during the second week when the trunk movements are gradually reduced to a minimum, both in frequency and magnitude, while movements of the legs become most prominent and active, it is the legs rather than the trunk which will initiate movements of a massive nature. In fact, between the 10th and the 13th days of incubation, in many cases, the so-called massive movements may be of such a nature as not to involve the trunk at all.

11. *The effect of previous movements.*—In the chick, other things being equal, if the embryo has just performed a series of more or less massive movements, its next movement within an interval of a few seconds is very likely to be localized. On the other hand, if the embryo is at rest, or is engaged only in slight or inextensive movements, there is greater possibility for its next response to be more massive or more involved.

The above are the factors that have been so far analyzed. There may be other factors which may also determine the extent of spread or local restriction of a movement. For instance, the amount of neuro-hormone liberated in a given stage or changes in the irritability or sensibility of the neuromuscular tissues to stimulation and hence changes in threshold during development may tend to alter the number of body parts to be involved in a response to stimulus of a given intensity. However, facts such as have been summarized in the previous paragraphs are already sufficient to emphasize the fact that embryonic movements are extremely complicated and variable and to warn us against the danger of oversimplified theories of development such as the total pattern theory and the reflex concept.

Summary and Conclusions

1. In this paper the two schools of thought regarding the development of behavior, the reflex theory and the total pattern theory, have been critically reviewed from both anatomical and physiological standpoints.

2. The anatomical evidences of the developing nervous system presented by the proponents of both schools fail to establish one theory to the exclusion of the other. Indeed, structurally, the embryonic nervous system may be regarded as neutral in that it does not predetermine whether development will proceed according to the Coghillian sequence or according to the reflex theory. Moreover, physiological interpretations on the basis of anatomical facts are rather speculative and may fail to give a true picture of the functioning of the nervous system.

3. It was pointed out that the present methods and techniques for physiological studies of the developing embryo are very crude, especially in the case of the mammalian fetus.

4. The criteria for the distinction between myogenic and neurogenic movements and for reflexes were also found to be of little value for scientific purposes.

5. Total pattern and local reflex as two distinct categories of embryonic behavior are inadequate for scientific description, as embryonic motility varies from time to time and from stage to stage in complexity and cannot be classified into such simple types. In other words, total pattern and local reflex are merely theoretical abstractions and represent the two extreme types of behavior; in between there are other movements of varying degrees of complexity, none of which can be strictly grouped under either the category of total pattern or that of local reflex.

6. Various factors which determine the extent of spread of a movement over other parts of the body or the extent of local restriction have been summarized. These factors serve to emphasize the variability and complexity of embryonic motility.

7. Neither the reflex theory nor the total pattern theory

gives us an adequate picture of the origin and development of behavior. The chief difficulty with both theories lies in their oversimplification of the developmental processes. Indeed, development does not seem to take place in such simple and uniform fashion as Barcroft and Windle or Coghill and Angulo have suggested. The three stages of behavior development classified by Barcroft, Barron and Windle (namely "the first, in which movements of individual parts of the body are quite isolated; the second, in which two or three such movements are associated into a group; and the third, in which such groups are integrated into a general mass movement") (6, p. 78), the Coghillian sequence of development, and the three steps proposed by Angulo (namely, myogenic, neuro-motor and sensory motor) should all be regarded as generalizations based on insufficient evidence. Such generalizations are due to the desire on the part of the theorists to devise uniform theoretical schemes without sufficient facts to support them. They may appear logical but are hardly biological, for development does not seem to be merely a process of fusing smaller units of movements into larger groups, nor that of isolating local movements from total pattern actions.

8. I have pointed out that the physiology of the embryonic nervous system is such a complicated and complex affair that it does not fit in with any of the existing theories of development. I have pointed out also the inadequacy of our present techniques and the need for more refined and quantitative methods for physiological studies of embryos.

In physiological embryology the quantitative aspect of both stimulus and response cannot be overstressed. In view of the variability and complexity of embryonic motility it is only through quantitative analysis that we can hope to be able to arrive at certain general laws regarding the intricate relations of developmental behavior.[3]

[3] While this paper was in press the writer succeeded in tracing the developmental history of acetylcholine in the chick embryo. This substance can be found as early as the 3rd day of incubation, or, in many cases, more than 24 hours before any external movement, spontaneous or in response to exterogenous stimulus, is first perceptible. Furthermore, although the work has not been completed as yet, there is already clear indication that electrical stimulation tends to increase the amount of acetylcholine

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output in the embryo. If the presence of acetylcholine in the embryonic tissue can be taken as an indicator of neural activity, its implication in connection with the problems discussed in this paper must be obvious: (1) on the basis of anatomical development one would hardly suspect that the nervous system in the chick embryo begins to assume functional activities at such an early stage. This serves to demonstrate clearly the danger of any attempt to interpret the origin of neural functions on the basis of anatomical development. (2) It is in direct contradiction to the myogenic view, which assumes that in the development of externally observable motility purely muscular, *i.e.*, non-nervous, movements necessarily antedate movements of a nervous nature since neural activities as indicated by the presence of acetylcholine begin long before there is any sign of observable muscular movements. And (3) the increase of the acetylcholine output by electrical stimulation may serve as a clear evidence against the view that the early responses of the embryo to electrical stimulus are myogenic in nature.

In a more recent investigation on the responses of the chick embryo to electrical stimulation by a much more refined quantitative method, in which both voltage and amperage for each stimulus were carefully measured and the threshold of response determined, and in which only a very small needle electrode (made from the smallest injection needle—gauge 27) was used, it was found that the intensity and time interval were important factors in determining whether a given response would be localized or spread, thus confirming the results by cruder methods of stimulation reported in this paper and elsewhere (35).

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WORK METHODS: AN OFTEN NEGLECTED FACTOR UNDERLYING INDIVIDUAL DIFFERENCES

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Current explanations of the origin of individual differences in human abilities tend in two main directions, (1) a dependence upon certain *biological bases*, such as the most favorable anatomical structures for effective physiological functioning of sense organs, nerves and effectors, and (2) a dependence upon *amount of previous training* (sometimes indirectly through extent of environmental opportunity for learning). To these two factors we may well add an intermediate factor, namely, (3) the particular *work methods* or patterns of behavior which are adopted by each individual in the course of utilizing his biological equipment during learning. Learning consists of (1) the preliminary adoption of various work methods, sometimes discarded in favor of others, and (2) the refinement of the final work method by such processes as overlapping of component parts, or redintegrations (conditioning of responses to some part of the situation instead of the entire situation as was necessary in early stages). It is quite possible that (2) is really a variation of (1). In measuring individual differences it is not sufficient to control the instructions or working situation, for the observer's previous incidental background may lead him to adopt very different work methods from those expected. It follows that 'control' limited to ordinary instructions and demonstrations is incomplete, and that other unnoticed factors operate to modify the work method actually adopted. The actual kind of work method itself must be observed, recorded, and analyzed if we are to have any adequate understanding of the influence of training. Our studies of individual differences have all too

frequently been carried out with inadequate qualitative analysis.

The terms work methods or behavior patterns are here used to include any variation in set, attitude, approach, trick of the trade, adjustment mechanism, etc., in other words qualitative variations in *ways* of reacting to a situation. The observer may frequently be unaware of the nature or plan of his work method. Probably the best known example of the importance of work methods comes from the photographic motion studies of Gilbreth (7) who reduced the average number of movements in laying a brick from 16 to 4, and thereby helped to lay the foundation for a new applied science of time and motion studies. These have become a recognized service of specialized firms in industrial engineering but have received little attention from psychologists. Slow motion pictures now afford an excellent method for such investigations, and are also widely used in athletic coaching, to diagnose correct and incorrect playing form and strategy.

The purpose of this paper is to show how these work methods are important in determining individual differences in *all* psychological activities, including *sensory*, *affective*, and *intellectual fields* as well as the *motor fields* from which our first illustrations were taken. The concept of work methods is by no means limited to serial or complex performances but applies equally to even the simplest performances such as sensory threshold discriminations and simple reaction times. The range of possible behavior patterns is determined in part by the structural limitations of the particular species or individual but it is also a matter of learning, often of the informal trial-and-error variety, and only vaguely understood even by highly skilled performers.

According to our analysis learning includes (1) the discovery or development of work methods, and (2) the perfection of the particular methods by such processes as overlapping of component processes and the development of 'higher units,' such as responding to words or phrases instead of separate letters as in typing, telegraphy, and piano playing. In some cases the essential work method is hit upon during

the early stages of learning and subsequent practice results largely in the improvement of details for greater speed and precision of efficiency. In other cases nearly the whole course of learning is concerned with a shifting from one method or set of methods to another and only in the last stages do the effects of repetition become important for polishing details.

It is not the purpose of this paper to deny the inheritance of individual differences in anatomical or physiological development. Both inheritance and environment are well demonstrated as determinants of such basic biological factors. It is likewise agreed that these variations in biological makeup are important as one set of factors determining individual differences in psychological activities, but we need to know more of their relative importance in comparison with the factors of the particular work methods adopted and the subsequent improvement of the method by repeated practice.

If our hypothesis as to the importance of work methods is correct then we cannot simply control amount of training and attribute all else to biological capacities as has been attempted so often in the past. The third variable, the *work methods employed*, is the unknown factor which we shall seek to evaluate in relation to the other two.

The importance of work methods may now be stated as follows: In responding to a given task an individual may utilize his anatomical and physiological characteristics in *a number of more or less equivalent patterns* of behavior. These various patterns (attitudes, approaches, tricks of the trade, adjustment mechanisms such as sublimation, etc.) may involve the use of the same body structures but frequently emphasize the importance of some other structures. Where a new pattern of action avoids the use of some body structure or functional mechanism in which the individual is deficient it may easily compensate for this deficiency. The patterns also differ widely as to their effectiveness in terms of the end results, such as fineness of sensory discrimination, adequacy of emotional adjustment, or effectiveness in terms of speed or precision of work performed. In simpler terms this theory

would emphasize not so much "what you are born with," as "what you do with it." In any given case, however, the variations in biological equipment might be of either greater, equal, or less importance than the behavior pattern in determining end results.

To use a mechanical analogy, the *capacity* of a simple mechanism such as a pump can be predicted from a knowledge of its measurements because except for minor variations the pump can usually operate in only one fixed way. The corresponding capacity of the human mechanism *cannot* be predicted unless the particular mode of operation is also clearly specified and controlled. The human mechanism is sufficiently complicated to permit *many different modes of operation.* Furthermore, the results of introspective analyses and motion studies indicate that even the most carefully standardized instructions and controls of psychological experiments only partially guarantee the selection of a given mode of operation, and in fact, frequently result in indirect complications which may not even be suspected unless the usual quantitative methods are supplemented by qualitative analyses such as introspection.

Let us now examine the evidence from the major fields of psychology to evaluate the relative importance of these three types of determinants, the biological capacities, the patterns of behavior, and quantitative variations (*e.g.* overlapping of movements, coordination of simultaneous operations, etc.) in the same general pattern of behavior.

In the sensory field, we find one of the largest bodies of experimental facts on individual differences in audition. The Seashore measures of musical talent provide six standardized experiments on the measurement of auditory discriminations. In this field we find very representative results on individual differences, *viz., wide variations* in individual thresholds, which are *relatively independent of the amount of previous training*, and *relatively stable* after even extensive amounts of professional musical training. Stanton's (24) careful experiments at the Eastman school of music have shown that such tests are valuable in predicting probable success or failure of students

during the standard course, and that four years of training in the school did not result in significant improvement in scores on the tests themselves. Thus far, the evidence would seem to be very favorable to a theory of biological capacities as determinants of acquired skill in a complex vocational activity.

It happens, however, that a number of incidental findings have arisen to suggest important limitations to the theory of biological factors, though they do not deny the results of this particular vocational guidance. First, we should expect that if there were all-important anatomical or physiological bases for individual differences in auditory discrimination thresholds, these should affect more than one type of discriminations. However, it is found that for a given age range, such as college students, the various measures of pitch, intensity, time, rhythm, consonance, and tonal memory are only slightly correlated and highly specific, with the single exception of pitch and tonal memory. Rhythm, for instance, is not predictable from the scores on time and intensity, a very difficult thing to explain on the basis of any known characteristic of either the sense organs or nerve structure. It is still possible, of course, that there may be a large number of biological correlates, one or more for each such functional difference. If this is the case, it would explain the low intercorrelations between similar tests, but it should be noted that none of the hypothetical biological correlates have yet been discovered for these various types of sensory discriminations. If true, it also vastly complicates both theory and practice of vocational guidance and personnel selection because of the enormous number of factors involved.

If we seek a learned basis for these individual differences, the observed intercorrelations between various types of auditory discriminations would be quite in keeping with the results on transfer of training in other fields, where fairly effective transfer is found in a few relatively narrow fields and little or no transfer between most activities, even those which in a qualitative way seem *a priori* to be almost as closely related as the tests which do fall within the narrow group

clusters. This point is to be treated in a parallel paper by the writer (**20**) on the factorial analysis of fine motor skills.

The previously cited findings of Stanton as to the absence of improvement in auditory acuities with extensive standard musical training may simply indicate that the particular types of training did not produce an effective transfer to the test situation. In a study at Oregon on the improvability of pitch discrimination under individual training specifically designed for that purpose, the writer (**19**) has in fact produced a rather significant amount of improvement within a limited period of time. Twelve students whose initial thresholds on the Seashore phonograph record of pitch were 30 d.v. or poorer, at a basal frequency of 435 d.v. (placing them roughly in the lowest decile on adult norms), were given from three to five hours of intensive training at or close to the thresholds of the individual on this same frequency. This small amount of specific training produced an improvement up to the average of 5 d.v. with a range of from 17 to 1 d.v. These figures, however, are not as significant as they appear to be because the normal average threshold is about 3 d.v. and the distribution is markedly skewed toward the superior levels. Its significance, however, is enhanced by the fact that it included all of the poorest scorers, and that class, theoretically at least, should be expected to include all those persons with the more serious anatomical handicaps, if such handicaps existed at all.

C. E. Seashore (**16**) had previously distinguished between *cognitive* thresholds, governed by ability in understanding the procedures, and *physiological* thresholds, which theoretically are determined by the biological limitations of the sense organ and nervous system. If this distinction be accepted, it should then follow that a good many of the poorer thresholds are due to cognitive factors and subject to improvement by the appropriate techniques. It is not yet known whether such intensive training on one note, 435 d.v., will transfer to other parts of the pitch range or whether each part of the range must be trained separately. Tone qualities and particular types of discriminative responses might also involve specific abilities.

In a further study we obtained preliminary introspections which strengthened the assumption that individuals differ as to work methods in activities even as simple as pitch discrimination. Some observers attempted to retain a clear auditory image of the first tone for comparison with the second, others employed a visual image of a vertical scale, still others attempted to reproduce the tone by implicit throat action, and so on. Further study on this problem is to be continued along with the studies on transfer of training in pitch discrimination. The crucial experiment will be to determine whether individuals who are instructed in superior work methods can significantly improve their own thresholds.

From these experiments we need not conclude that there are no physiological limits, but merely that these limits, if any, are valid *only for a given work method*, and that by the use of other work methods a significant improvement may be obtained. The results on the improvement of pitch control through use of visual cues in the Seashore tonoscope illustrate a work method in which improvement may be achieved by utilizing another structure, the eyes, in conjunction with the training of the principal structures of ears and proprioceptors.

In the field of affective processes, the term *adjustment mechanisms* has been applied to the various ways of solving emotional situations. Such mechanisms are synonymous with work methods in the other fields. Up to the present time objective measurements of individual differences in emotional adjustments have been stated in terms of hypothetical *traits* such as introversion-extroversion which are inferred from the occurrence of certain symptoms, or end results.

Recent analyses of I-E tests have shown however that nearly the same quantitative scores (end result in terms of number of symptoms) may be produced by very different mechanisms having very different significance for adjustment to practical situations. Thus an individual may attain a given I-E score by being primarily interested in abstract types of work, or by fear of social contacts, or still other causes. The partial or complete failure of I-E tests to predict success in various practical situations seems quite logical in view of

the diversity of underlying behavior patterns which may be involved, even though these mechanisms are sometimes causally related.

Personality inventories have usually emphasized classifications based on addition of numerous end results or *symptoms* which are considered *a priori* to be indicative of certain types of emotional adjustment. The qualitative study of the adjustment mechanisms underlying conflicts or frustrations, *e.g.* compensation, rationalization, substitution, sublimation, etc., which has recently been extended by Lewin, Shaffer, Wallin, Hull, and others has been almost entirely overlooked as a source of objective test construction, but the writer and Katz (**21**) have presented a classification of such adjustment mechanisms which goes deeper than the current symptomological approach.

The basic theory here presented suggests that in proceding toward a goal, an individual may be blocked or frustrated in his drive by some other conflicting drive, social custom, or environmental obstacle. In this conflicting situation, he is faced with a number of alternative procedures or solutions of the problem. These solution mechanisms are equivalent to the *work methods* in other fields, *e.g.*, motor skills. If the drive is weak, the individual may simply turn aside and give up the particular goal in favor of another goal (*substitution*). Even if the drive is strong he may see no possible solution and give up. If the drive is a continuing one, giving up is only temporary, and he will later have to either strongly *repress* it, *regress* to an earlier, perhaps even an infantile or sick person's type of solution (sympathism), respond negatively by refusing to recognize the situation or conventional solutions of it (negativism), or revert to imaginary solutions (phantasy, or Lewin's irreal solutions).

If the drive is strong, he may simply redouble his energy in the original type of behavior (compensation) which may go so far as to be called over-compensation, often becoming ridiculous socially. A second type of compensation is that which transfers to another activity, as when a dull student attempts to compensate in athletics or some other outside

activity. Various other mechanisms include *rationalization*, *egocentrism* (attention getting), *identification* with some more successful persons or groups, and dissociation or the use of 'logic tight compartments' in refusing to recognize the inconsistency of separate adjustments to each of the conflicting forces.

Each of the twelve qualitatively different adjustment mechanisms (behavior patterns) for the solution of emotional problems can also be rated quantitatively on its effectiveness as a work method with respect to (1) the extent of achievement of the individual's original goal or a satisfactory substitute, (2) the attainment of social approval or disapproval as a secondary goal, (3) the usefulness or undesirability of the behavior to his social group, and (4) the building up of habits of effective or ineffective adjustment mechanisms for the solution of future conflicts.

Two or more mechanisms which are qualitatively very unlike in some instances achieve practically equivalent *end results* in terms of these four criteria, but their probabilities of attaining the desired results vary widely from one mechanism to another. This hypothesis of different mechanisms is quite different from the simpler idea often implied in personality adjustments that we differ mainly in quantitative aspects of the same hypothetical larger pattern of behavior, *e.g.*, 'neuroticism.'

Another value arising from the analysis of such affective behavior patterns is the fact that the mechanisms can be simply described in terms of the individual aims, behavior, and results. Alternative solutions may then be presented in terms of other mechanisms which are equally understandable in terms of *what to do* and their probable results.

The continuity between these normal adjustment mechanisms and the psychopathic behavior which results from their exaggerated development suggests also the basis for the psychiatric classification of mental disease based upon underlying mechanisms in place of the current symptomological classifications which are widely recognized to be unsatisfactory.

Various typological theories have suggested a correlation between, *e.g.*, body build and personality characteristics. The principal theory in this field, that of Kretschmer, seems to be largely invalidated by the quantitative studies of Klineberg, Asch, and Block (10) who found that what little evidence there was could be explained upon the basis of changes in body proportions with increasing age. It is not at all necessary that all such biological bases for affective characteristics be disproved. On the contrary, it is quite possible that some such bases as glandular balance may be quite important even within the normal range of variation. It may be significant, however, that to date few of the possible relationships which have been studied have been verified.

Even when biological bases are discovered, however, it is still important to find whether the end result is mediated by *different degrees of the same type of behavior or by qualitatively different mechanisms* with overlapping ranges of effectiveness.

In the intellectual processes we have a considerable body of experimental evidence as to the relationships between possible biological bases and functional effectiveness, *e.g.*, in tests of 'general intelligence.' The work summarized in Paterson's *Physique and Intellect* (13) indicates that except for a few pathological cases at the lower extreme of the distribution, there is no significant correlation between intelligence and the body size, height-weight ratio, head size, brain weight, or other structural measurements. Certain neurologists believe that they can distinguish structural differences in the cortical cell development of feebleminded and highly intelligent individuals, but this point is still controversial, and even if valid for distinguishing pathological cases on the lower extreme of the distribution it is by no means certain to be the basis for the large differences within the main portion of the normal distribution of intelligence scores.

The lack of correlation between intelligence and any known characteristic of size, proportions or weight of the brain is perhaps not so disturbing as the corresponding lack of any

known correlation with a functional aspect of the nervous system such as speed of conduction. This extensive literature has been reviewed by Farnsworth, Seashore, and Tinker (**8**), but one of the more recent studies may be cited to illustrate the total lack of correspondence. Travis and Dorsey (**26**) have shown that the time for neural conduction in the knee jerk (electrical action current technique, eliminating the slower muscular component) is no faster for male inmates of a feebleminded hospital than for superior children of comparable age 7–10 yrs. When no difference is found between such extremes of a distribution it becomes very unlikely that any will be found anywhere within the normal range of intelligence. However, the idea is a favorite among many psychologists and continues to crop up at rather frequent intervals, in disregard of the large body of evidence against it.

Perhaps the strongest evidence for some neurological basis for intelligence is found in superior families where the best of individual care given to a feebleminded child has been insufficient to produce any significant improvement over a period of years. At least some of these cases are definitely accounted for as cases of birth injury or other definite physical handicaps such as glandular imbalance in cretinism. Harlow (**8**) has recently reviewed the field of neuro-intellectual correlations.

It may be that outside of these cases where normal anatomical growth has failed or degeneration has set in there are also cases of individuals whose nervous systems contain the normal number and arrangement of nerve cells in healthy condition, but who have not yet learned efficient work methods for the solution of verbal problems such as are found in most intelligence tests. The results of the Wellman experiments (**27**) on pre-school children at the University of Iowa indicate that moderately superior children do increase very significantly in measured intelligence as a result of continued attendance in superior school environment, and that control groups placed in ordinary schools do not show this improvement. Skeels and Fillmore (**21**) also showed that children placed in a state institution, which evidently constitutes an inferior environment, likewise show significant

decreases in measured intelligence as a result of continued stay in this environment. They also showed that illegitimate children of mothers of low I.Q.'s develop superior I.Q.'s when raised in superior foster homes. Furthermore Skodak (22) has shown that illegitimate children of a group of definitely feebleminded mothers did not show feeblemindedness themselves but were approximately normal when raised in superior foster homes.

Again, one need not conclude that hereditary or any other biological bases are of no importance, but merely that the range of possible improvement in intelligence in any given individual is considerably greater than we have previously believed.

The improvability of intelligence test scores might be considered to represent either increasing speed or effectiveness in the same work methods, or the adoption of superior work methods or both. One of the interpretations of the historical imageless thought controversy is that the two sides simply found observers having individual differences in methods of thinking, some with strong imagery and others without it, or at least unable to verbalize about it without training such as Titchener gave in introspective procedures. Ruger's (15) classical study on the solution of mechanical puzzles has indicated some of the specific attitudes, cues and work methods which were found to be most effective in solving such problems. A somewhat similar study by Husband (9) indicated that in human learning of a lengthy finger maze there was so much difference in the effectiveness of different methods that practically no one succeeded in learning it by either rote kinesthetic or visual imagery methods, but instead everyone eventually hit upon a verbal method of counting, and the sooner an individual adopted this method the sooner he learned the maze. Various sub-types of the verbal method also appeared, with varying degrees of effectiveness.

The author (18) has recently shown that in the solution of the simple disc transfer or pyramid puzzle, students exhibited all degrees of insight from an almost random trial-and-error, through vague unanalyzed hunches, to visually imagined

series of steps and on up to verbal rules of various degrees of adequacy and completeness. In the most difficult levels of the problem neither overt trial and error nor casual insights were anywhere nearly as effective as a formal inductive method of analyzing the major variables, systematically recording and classifying the data, and examining the summaries for recurrent patterns or principles underlying solutions. The technical methods of solving intellectual problems in the various sciences, professions, and trades are of course excellent examples of specialized work methods.

The principal theories as to the origin of individual differences in motor skills have been summarized by the writer (**17, 20**). Within the group of high speed manual skills low correlations of about .20 are the rule, and the only significant correlations occur in very restricted groups, *e.g.*, one group of visual and auditory reaction times, and two sub-groups of tapping tests which however are relatively independent of each other and of simple reaction time. The fact that different musculatures, *e.g.*, hand, jaw, foot, are closely intercorrelated in a single skill, *e.g.*, reaction time, is of no help in the practical problem of predicting success in other more practical skills. It is therefore difficult to postulate any general neuro-muscular factor or even any group factors of significant breadth which might account for the observed large and relatively stable individual differences in various skilled manual speed performances.

As a crucial test, Buxton and Humphreys (**3**) tested and then further trained a larger number of subjects on four motor tests including the two different kinds of tapping tests until plateaus were reached on the learning curves. If there were some fundamental biological factors such as speed of nerve conduction or muscular contraction operating as determinants of skill in movements of the right arm these physiological limits should appear in both types of tapping tests after the extensive training. Instead of the expected rise in correlations from the pre-training to the post-training series there was actually a slight though insignificant decrease in five of the six correlations and no significant change in the other.

So far as the writer can see, the only remaining type of hypothesis for a biological basis of individual differences in motor skills is to suppose that there is a different set of biological factors underlying almost every separate test, or at least for each narrow group, as in the tapping tests.

The work of McNemar (11) on the intercorrelation of motor scores on identical and fraternal twin boys does in fact support such a theory although it can also be interpreted as at least partially due to similarities in training of the twins. This important experiment should be extended and varied to determine more exactly its real significance. The principal reasons for questioning or minimizing the biological interpretation are that no known biological basis has been suggested, and that practically all of the other evidence in this field of manual speed skills tends to minimize its possible importance in favor of the explanation of differences in work methods.

The classic experiments of Book (1) on typewriting suggested a compromise interpretation which seems most reasonable, *viz.*, a given set of physiological limits, *e.g.*, maximum speed of muscular contraction, determine the end result *only for a given work method. If other work methods are introduced* so as to minimize the importance of the limiting factors then further progress may follow. The classic learning plateaus found by Bryan and Harter (2) in the acquisition of motor skills in telegraphy were, in fact, attributed to successive changes from single letter to syllable, word, and even phrase grouping patterns. The classic tachistoscopic experiments of Cattell on 'higher units' of perception, and overlapping of processes likewise fit closely into this explanation. (*Cf.* Dearborn (5), for an interesting summary of these experiments presenting views similar to those of the present writer.) For example in a range of attention experiment, ten unrelated letters cannot be accurately reported, but ten letters in the word UNIVERSITY represent a higher (learned) unit which can be reported with little difficulty. Hollingworth's theory of integration suggests that a single cue comes to evoke a pattern of response which originally was evoked only by a

whole situation. Such a change in method may also be analogous to shifting to a higher gear in driving an automobile. The inductive methods of scientific research might then be thought of as a super 'low gear' having tremendous power, but with low speed.

The historical collapse of mental chronometry in the 1890's further illustrates a major difficulty of explaining complex performances on the basis of physiological limits. Thus, if there are five steps in unlocking and opening a door (*e.g.*, extract key ring, select key, insert in lock, turn knob, push door), the sum of the minimum times for each step will not predict the minimum time for the total movement because the steps can be so greatly *overlapped* as to obscure the importance of each element. Lange, Ach, Messer, and others mentioned by Murphy (**12**), have shown by introspection that various attitudes or 'sets' produce significant variations in reaction time. The order from slowest to fastest was the sensory attitude (attention to stimulus), central (or divided attention), and motor attitude (attention primarily to making responses). It was further indicated that inexperienced observers often change from the sensory to the motor attitude after extensive practice, which is usually accompanied by some improvement in reaction time.

Probably the simplest example of the significance of work methods is given by Perrin (**14**) in an experiment on speed of card sorting. Simple observation revealed that a number of techniques or 'tricks of the trade' were employed by certain of the observers, *e.g.*, (1) holding the deck in the left hand *close* to the sorting trays rather than several inches away, (2) edging the top card off with the left thumb so as to be easily grasped by the right hand, (3) moving the card toward the trays while turning it over for inspection, and (4) tossing the card rather than placing it on the trays. The superior half of the group, as judged by average speed, utilized a larger number of these techniques than did the slower half. Such casual observation could not of course be expected to show complete differentiation of the groups and in fact one member of the slower half of the group used all of these techniques and

still did not get above average. This might be attributed to the absence of still other unnoticed methods, and perhaps to an incomplete degree of overlapping of the movements. It is only fair to add that it might also indicate that the combination of the best methods might not have been sufficient to completely overcome the hypothetical physiological limits which might have been involved in the test.

The clearest and most extensive evidence for the importance of work methods comes from the well-known but largely neglected findings of Gilbreth on the elimination of useless movements. Such studies have been repeated and extended in large numbers of other motor skills, principally by industrial engineers, but unfortunately the results have attracted very little attention in psychology, perhaps because the results are frequently unpublished trade secrets. Gilbreth later checked his analyses by constructing wire models of the hand movements of skilled and unskilled operators to show the difference in techniques to beginners and to aid in their training along the most effective lines. Various minor points have arisen, as to whether there is *one best way* for all persons to use, and, even if there is, whether it can be used continuously without undue fatigue. These, however, are trivial questions, and the evidence is clear that such analysis of work methods can and does produce very significant improvements in motor skills. Slow motion pictures have now largely supplanted Gilbreth's earlier methods of still photography, but the principles are much the same. Gilbreth developed a standard terminology of 'therbligs' or unit types of best movements as a shorthand method for describing various types of skilled movements, but this need not imply any neglect of the factor of overlapping of the movements.

Cox (3) has shown that whereas training in one type of factory assembling operation did not transfer to a second similar operation under ordinary conditions it was possible within the same length of time to introduce efficient work methods on the first skill which did produce very marked transfer to the second operation. Furthermore the workers trained in superior methods continued to become even more

superior as time went on. This study is of the greatest importance for applied psychology in all fields.

The significance of these sample results on the analysis of sensory, affective, intellectual, and motor skills may be summarized in a few sentences. (*1*) *Individual differences in attainment of various practical skills may be in part determined by anatomical or physiological limitations of the individual's receptors, connectors and effectors.* (*2*) *Such limitations, however, apply to specific behavior patterns or work methods including attitudes and 'sets' as well as the main activities, and* (*3*) *changing to a different behavior pattern may partially or completely overcome any one or several of these limiting factors, thus permitting further progress in learning of the new skill.* These changes occur even from trial to trial.

Such a view helps to explain the very commonly observed specificity of individual differences in human abilities which placed so great a strain on theories of biological determinants of aptitudes. The concept of qualitative differences in behavior patterns or work methods is in keeping with both the quantitative findings on individual differences and the related fields of learning and adjustment.

An adequate recognition of work methods further minimizes the present 'fatalistic' attitude of vocational guidance, that a person is biologically endowed with certain basic aptitudes for success or failure in various fields. In place of this fatalistic theory it suggests that while these limitations may exist for any given work method, they may be overcome by employing a different work method which is less subject to these same limitations. This fact has been recognized in a recent study by Thurstone (**25**). Qualitative analysis has already indicated the nature of the preferred methods in certain fields, and the same type of experimental methods, principally verbal report and photographic motion study, can be used wherever needed for other fields.

Selection of workers on the basis of initial tests may still be useful in certain fields where learning is very difficult, but in many others fields vocational guidance should come to emphasize more and more the possibilities of attaining a given

skill by experimentally evaluated work methods. Furthermore, the real significance of biological factors underlying a given skill can only be determined by controlling the other major variables of behavior patterns *as well* as the amount of previous training, both formal and informal. Such behavior patterns may even in many cases be adopted as a result of unrecognized chance discovery, as in golf, where the behavior pattern is frequently not well understood even by the skilled professional performer himself. The discovery and teaching of such effective work methods should henceforth become one of the major functions of the applied psychologist in all fields. Gestalt psychology has so far emphasized the idea that these important patterns simply appear as forced reactions to the total situation. Consulting engineers and teachers interested in diagnostic learning have long shown however that such patterns can be analyzed and redirected as an effective aid to learning.

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TESTS OF SIGNIFICANCE FOR EXTRA SENSORY PERCEPTION DATA

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Introduction

The following statistical discussion was prepared to meet the needs of experimenters in extra-sensory perception (clairvoyance or telepathy). The results are, however, probably applicable to other types of experiment. Care has been taken to make the statistical argument intelligible to a comparatively non-mathematical reader, and no reference to outside sources is necessary.

Experiments, which have been made extensively with startling results in the United States and no results in England, follow a common pattern. A pack consists of cards bearing one of a number of simple symbols. In the usual ESP pack, there are 25 cards with five of each of the symbols: circle, cross, rectangle, star, and wavy lines. The pack is shuffled, and the subject guesses each card in order, either card by card as they are taken off the pack, or straight through as the pack stands. His score is the number of correct guesses, and a significantly high score may indicate telepathy, clairvoyance, or fraud, according to the conditions of the experiment.

Statistical tests on extra-sensory perception data are made with the aid of two quantities, (*a*) the expected number of 'hits' (correct guesses), (*b*) the variance (squared standard error) of the number of hits, both calculated on the null-hypothesis that guesses are uncorrelated with presentations. An observed score exceeding expectation by more than twice the standard deviation may then be judged significant.

The simple method of determining these quantities is to

assume, not only that guesses are uncorrelated with presentations, but also that the subject is on each occasion equally likely to guess any of the possible symbols. This leads, in the common experiment, to an expectation of five hits, with variance four. The assumption however is certainly false, since definite symbol 'preference' has been demonstrated (rectangles are unpopular).

The expectation and variance will be correctly determined, if we regard the experiment as equivalent to the matching of two randomised packs, one to represent the presentations, and the other to represent the guesses. In the particular case when there are five cards each of five types, the expected number of hits is still exactly five, whatever the distribution of guesses, and the variance is usually not much different from four (1).

It seemed to the present writer that the convention of equal numbers of presentations of the possible types, places an unnecessary restriction on the technique of the experiment. For example, a test might well be made by asking the subject to call out the last figures of a series of 10-figure logarithms, in which case the numbers of each of the ten digits 0, 1, 2, $\cdots$ 9 are not exactly equal. Again, even with equal numbers of presentations, it might be desired to permit the subject to leave a blank on occasions when he thinks his impressions are too 'confused.'

We were therefore led to solve the completely general matching problem, in which the numbers of cards of each kind are unequal in both packs (3). In the present paper, the argument has been simplified, so that although the problem is now in its most general form, the method is, we believe, clearer than in the published proofs for the special case.

Statement of Problem

Two packs each contain N cards, which are of n different types, $T_1, T_2, \cdots T_j, \cdots T_n$. The packs contain respectively the following numbers of the n types:

Type	T_1	T_2	...	T_j	...	T_n	Total
Pack A.....	a_1	a_2	...	a_j	...	a_n	N
Pack B.....	b_1	b_2	...	b_j	...	b_n	N

Any of the $2n$ numbers a_j or b_j may be zero. In the usual ESP pack, for example, we have

$$N = 25$$
$$n = 5$$
$$a_1 = a_2 = \cdots = a_5 = 5.$$

The packs are shuffled and laid down in two rows, one below the other. The positions in a row will be called the 1st, 2nd, $\cdots$ rth, $\cdots$ Nth place. A hit is scored in any place, if the two packs have cards of the same type in that place. The total number of hits will be denoted by x. It is desired to find the expectation and variance of x.

Expectation

In the argument which follows we shall suppose that cards of the same type are rendered distinguishable from each other. The condition of shuffling, or randomisation, implies that all possible arrangements are equally likely.

We proceed to find the probability that a hit occurs in the rth place. A hit is scored if the card of both packs is of type T_j. The probability that the first pack has type T_j in the rth place is clearly

$$a_j/N, \tag{I}$$

since there are N cards which might have gone into the rth place, but only a_j cards of type T_j. Similarly the probability that the card of the second pack is T_j is b_j/N. Multiplying the two independent probabilities together, we find that the probability that both cards in the rth place are T_j is

$$a_j b_j/N^2. \tag{II}$$

The two cards might however have been any of the n possible types. Hence the probability of a hit in the rth place is

$$\frac{1}{N^2}\sum a_j b_j. \tag{III}$$

Unless otherwise marked, Σ denotes summation over all values of j from 1 to n. There are N places in which a hit might occur. Hence the expected number of hits is found by multiplying the above probability by N. Therefore the expectation of x is

$$\bar{x} = \frac{1}{N} \Sigma a_j b_j. \qquad \text{(IV)}$$

The bar over the x denotes the expected value of the quantity beneath the bar.

Variance

The variance will be evaluated by a trick which often proves effective (*e.g.*, **2**). We shall find the expected number of ordered pairs of hits. For example, six hits contain $6 \times 5 = 30$ ordered pairs of hits, since we may take any of the six hits as the first of a pair, and any one of the remaining five as the second of the pair. In general, if there are x hits, there will be $x(x-1)$ ordered pairs of hits. The expected number of pairs of hits is

$$\overline{x(x-1)}$$

and may be recognised as the second factorial moment of the distribution of x.

We therefore proceed to find the probability that two specified places, say the rth and the sth, are both hits. Two cases can be distinguished:

(*a*) The pairs of cards in the two places are of the same type T_j.

(*b*) The pairs of cards are of different types T_j and T_k.

Hits of the same type.—The probability that the first pack has a T_j in both the rth and the sth places is

$$\frac{a_j(a_j-1)}{N(N-1)}, \qquad \text{(V)}$$

since there are $N(N-1)$ ways of filling the two places with any cards, but only $a_j(a_j-1)$ ways of filling them with cards of type T_j.

Multiplying this by the similar expression for the second pack, we obtain the probability of hits of type T_j in both places;

$$\frac{a_j b_j (a_j - 1)(b_j - 1)}{N^2(N-1)^2}. \qquad \text{(VI)}$$

Hits of different types.—The probability that the first pack has a card of type T_j in the rth place and of type T_k in the sth place is

$$\frac{a_j a_k}{N(N-1)}. \qquad \text{(VII)}$$

Multiplying by the similar expression for the second pack, we obtain the probability of two hits of types T_j and T_k respectively in the rth and sth places.

$$\frac{a_j b_j a_k b_k}{N^2(N-1)^2}. \qquad \text{(VIII)}$$

Summation of probabilities.—The probability of a pair of hits of the same type is given by the expression (VI) summed over all values of j from 1 to n.

$$\frac{1}{N^2(N-1)^2} \sum a_j b_j (a_j - 1)(b_j - 1). \qquad \text{(X)}$$

The probability of a pair of hits of different types is given by the expression (VIII), summed over all values of *both* j *and* k from 1 to n, with the restriction that j is not equal to k.

$$\frac{1}{N^2(N-1)^2} \sum_{j \neq k} \sum a_j b_j a_k b_k. \qquad \text{(XI)}$$

The probabilities for the two cases may now be added together. We may write

$$a_j b_j (a_j - 1)(b_j - 1) = a_j b_j a_j b_j - a_j b_j (a_j + b_j) + a_j b_j \qquad \text{(XII)}$$

and hence use the terms $a_j b_j a_j b_j$ to replace those which have been excluded from (XI). This gives

$$\sum_j \sum_k a_j b_j a_k b_k, \qquad \text{(XIII)}$$

where the double summation takes place over all values of j and k without restriction. The double sum is therefore equal to

$$\{\sum a_j b_j\}^2 \qquad \text{(XIV)}$$

and the probability of the simultaneous occurrence of hits in the two specified places is

$$\frac{1}{N^2(N-1)^2}\{(\sum a_j b_j)^2 - \sum a_j b_j(a_j + b_j) + \sum a_j b_j\}. \qquad \text{(XV)}$$

The number of ordered pairs of places, which might have been considered, is $N(N-1)$, and hence, multiplying the above expression by $N(N-1)$, we find the expected number of pairs of hits

$$\overline{x(x-1)} = \frac{1}{N(N-1)}\{(\sum a_j b_j)^2 - \sum a_j b_j(a_j + b_j) + \sum a_j b_j\}. \qquad \text{(XVI)}$$

An obvicus extension of the preceding argument gives the higher factorial moments, if these are ever needed.

Variance.—The variance of x is defined to be the expected value of the square of its deviation from the mean. This is equivalent to the mean value of the square, minus the square of the mean value, which in turn may be written as

$$\overline{x(x-1)} + \bar{x} - (\bar{x})^2. \qquad \text{(XVII)}$$

Substituting for $\overline{x(x-1)}$ from (XVI) and for $\bar{x}$ from (IV), we find

$$\text{variance } (x) = \frac{1}{N^2(N-1)}\{(\sum a_j b_j)^2 - N\sum a_j b_j(a_j + b_j) + N^2\sum a_j b_j\}. \qquad \text{(XVIII)}$$

Numerical Example

Lest it be thought that these expressions for expectation and variance are too complicated for practical use, we give an example to show how quickly and easily they are evaluated.

Type	I	II	III	IV	V
	Numbers of each type		Product a_jb_j	Sum $a_j + b_j$	$a_jb_j(a_j + b_j)$
	a_j	b_j			
1	4	0	0	4	0
2	5	10	50	15	750
3	6	6	36	12	432
4	3	7	21	10	210
5	7	2	14	9	126
Totals	25	25	121	—	1518

In columns I and II are listed the numbers of cards of each type in the two packs respectively. Column III contains the products, and column IV the sums of numbers in I and II. Column V contains the products of numbers in columns III and IV. Therefore

$$N = 25$$
$$\sum a_jb_j = 121$$
$$\sum a_jb_j(a_j + b_j) = 1518$$
$$\bar{x} = \frac{121}{25} = 4{\cdot}84$$
$$\text{variance } (x) = \frac{121^2 - 25{\cdot}1518 + 25^2{\cdot}121}{25^2{\cdot}24}$$
$$= 3{\cdot}48773.$$
$$\text{Standard deviation} = 1{\cdot}86755.$$

One Pack with Equal Numbers of Different Types

When we set $a_j = a$, all j from 1 to n, we find

$$\sum a_jb_j = a\sum b_j = aN$$
$$\sum a_j^2b_j = a^2N$$
$$\sum a_jb_j^2 = a\sum b_j^2.$$

Formulæ (IV) and (XVIII) for expectation and variance now reduce to

$$\bar{x} = a, \qquad \text{(XVIII)}$$

$$\text{variance } (x) = \frac{a(N^2 - \Sigma b^2)}{N(N-1)}. \qquad \text{(XIX)}$$

In the commonly used ESP pack of 25 cards, five of each of five types, we have

$$\bar{x} = 5, \qquad \text{(XX)}$$

$$\text{variance } (x) = \frac{625 - \Sigma b^2}{120}. \qquad \text{(XXI)}$$

We note that the expectation is now exactly five, whatever the distribution of guesses. The variance too, is generally not much different from the values (4 or 4.16) commonly employed. For example with the distribution of guesses

6 7 5 5 2

variance $(x) = 4.05$.

Tests of Significance

If the quantities a_j are the numbers of presentations of each type, and b_j the numbers of guesses of each type, the expectation and variance, calculated as above, are those of the number of correct guesses, allowing for the subject's preferences as revealed by the data themselves. The expectation is naturally enhanced or depressed, according to whether the subject's preferences run with or counter to the frequencies of presentations.

The test of significance is made by seeing whether the score exceeds expectation by an amount greater than twice the standard deviation (2½ per cent significance).

One would, however, not attempt to carry out a separate test of significance on so small a body of data as that in the numerical example. The data of all homogeneous experiments may be thrown together before any test is made. This means, at least, that the data from one subject in the course of a day, might well be thrown together. When it is necessary to combine scores from a number of different experiments or different subjects, the expectations and variances, separately calculated for each set of data, should

be summed to give the expectation and variance of the total score. If $a_1, a_2, \cdots a_n$ are constant for a large set of experiments, the computation may be greatly reduced by a method shown by Stevens (3).

Summary of Formulæ

	Expectation	Variance
Both packs with unequal numbers of the n types.....	$\frac{1}{N}\Sigma a_j b_j$	$\frac{1}{N^2(N-1)}\{(\Sigma a_j b_j)^2 - N\Sigma a_j b_j(a_j+b_j)+N^2\Sigma a_j b_j\}$
First pack with equal numbers (a) of the n types.....	a	$\frac{a(N^2-\Sigma b^2)}{N(N-1)}$
Guesses on ESP pack of 25 cards, 5 of 5 types.....	5	$\frac{625-\Sigma b^2}{120}$

Number of cards in a pack $= N$
Number of cards of each type in first pack $= a_1, a_2, \cdots a_j, \cdots a_n$
Numbers of guesses, or of cards in second pack $= b_1, b_2, \cdots b_j, \cdots b_n$
Σ denotes summation over values of j from 1 to n.

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ARISTOTELIAN, GALILEAN AND NON-ARISTOTELIAN MODES OF THINKING

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It is common knowledge that the several special sciences were once inhabitants of the domain of philosophy and that psychology was one of the last of the offspring to leave the ancestral home. No doubt in certain respects psychology has benefited by its recent separation from philosophy; and yet psychology certainly has had to pay a price for its newly-won independence. The fact that psychology was cut off from its formerly more intimate contact with logic has been particularly detrimental to the progress of psychology in its rôle as an up-and-coming science.

Fortunately this condition is now being corrected. In recent years we have seen evidence of a reconciliation between logic and psychology. This reunion arises in part out of the needs which have grown up within the field of psychology itself. Conflict between systems has suggested the advisability of an examination of the logical technique used in the construction of such systems. The return to a closer relation is also due to the influence of Gestalt psychology which, with its philosophical background, has brought theoretical considerations back into repute. To be sure, this tendency to reëxamine methodological procedure is manifested throughout the entire domain of science, from physics to social science, so that this present trend in psychology is not unique.

While in a general way the influence of Gestalt theory is to be commended, it is my purpose here to argue that Gestalt theory has not gone far enough in its examination of the rôle of logic in the construction of systems. The development I particularly have in mind is that represented by Kurt Lewin

in his recent effort to put psychology on the right path through the application of the concepts and technique of topology. In his book, *The Principles of Topological Psychology* (4), Professor Lewin presents the thesis that psychology is now at the point where physics was about three hundred years ago —at the time of Galileo and Newton—and that the next step in the evolution of the science of psychology will be taken when it enters upon its post-Galilean career. Part of the background of this thesis is provided by Lewin's contrast between what he describes as the earlier, Aristotelian mode of thinking, which hitherto has controlled psychological theorizing, and the later, Galilean mode of thinking, which psychologists must apply in their present systematizing efforts.

It is my understanding that in Lewin's view topology provides at least one way in which psychology may hopefully pursue its way toward the goal of Galilean systematization. This may be illustrated by one example. According to Lewin, one of the characteristics of the Aristotelian mode of thought is to find the explanation of events in terms of the individual as such, in some inner motive or emotion, whereas the Galilean view demands that we no longer seek the 'cause' of events in the nature of the single isolated object, but in the relation between an object and its surroundings (field). Only in terms of the Aristotelian presuppositions does the question of whether 'heredity' or 'environment' is more important possess a meaning. But topological psychology rejects this problem, for it recognizes that behavior is always a function of a person in an environment, or $B = f(P, E)$. And here topological psychology introduces *field* concepts to unite organism and environment.

In commenting on this proposed advance in psychology we here wish to propose that gestalt theory of this type needs to be even more radical in its critique of logical foundations. Several years ago we discussed the possibility that non-Aristotelian developments in logic might be of great significance for psychology (7). In the present paper this idea is reëmphasized and extended. In a review of Lewin's book in *Philosophy of Science* (Oct. 1936) we raised this ques-

tion: If psychology is now at the place which physics occupied at the time of Galileo and Newton, and if, as we know, Newtonian physics was succeeded by post-Newtonian physics, why may not psychology learn a lesson from physics and take a short cut, jumping directly into its post-Galilean stage? Indeed, we ask, *should not the more general development of a non-Aristotelian logic and science include and transcend the Galilean mode of explanation?*

That such a general attack would yield all the results already attained seems clear. For example, consider the case of the rejection by topological psychology of the organism-environment antithesis. This step has already been taken in logic through the attack upon and rejection of the Aristotelian notion of 'substance,' which was intimately tied up with the Aristotelian subject-predicate logic. The formulation of the view that qualities inhere in a thing-like core is rightfully attributed to Aristotle, for the categories of 'substance' and 'quality' first appear explicitly in Aristotle's system. The inseparable connection between Aristotelian metaphysics and Aristotelian logic, and the way in which both have influenced the development of science during twenty-three hundred years of Western European culture, are subjects which have not been sufficiently investigated, but it appears to me that it is possible to present a good case for the thesis that, following Aristotle, the entire theory of matter which has been at the bottom of all science since the time of the ancient Greeks is a consequence of the Aristotelian logic of classes. Here, in support of our thesis, we have time only to note that the foundation of Aristotelian logic is the doctrine that every proposition must affirm or deny a predicate of a subject, and since Aristotle's definition of primary substance is that which can be a subject but never a predicate, properties of substances must be qualities predicated of subjects. In other words, in propositions the subjects are represented by class names, and in a logic of classes the predicates are the ascription to, or denial of, qualities or attributes to the subject terms (substances). In the medieval realism of the scholastic thinkers this inner core of things was the *substantia essentialis*.

This notion of the 'thing' as the bearer of the qualities which inhere in a 'substantial' substratum has influenced all our thinking. It underlies all traditional forms of physics and psychology, so that Newton's theory of matter, Descartes' theory of the interaction of soul and body, and J. B. Watson's behavioristic psychology have all paid tribute to this mode of explanation. Today we have the modern electrodynamic theory of matter and this removes the last foundation-stone of the substance-quality (subject-predicate) logic. This eliminates the last vestige of scholasticism from physics. Thus we free ourselves from the shackles of the past and prepare for further advances.

Now compare this advance beyond Aristotelian logic with the parallel attainment of the Galilean mode of thinking as Professor Lewin has presented it. The non-Aristotelian logician rejects the philosophic and scientific consequences of Aristotelian logic by affirming that the traditional explanations are guilty of the *fallacy of the absolute individuality of the subject* (*substance*). In Lewin's 'dynamic theory of personality' there is a similar rejection of 'absolutes' and the individual is treated in terms of an integration of stresses and shifting forces. But non-Aristotelian system goes further and insists that in an evolving world, organically interrelated, the Aristotelian 'law of identity' (or principle of individuality) does not hold absolutely. From the 'lowest' level of nature, the level of electrons and protons, to the 'highest' level that we know, the level of human beings, everything is evolving and manifests whatever 'properties' it 'possesses' through its interactions with other 'things' in the cosmic environment. An atomistic-elementalistic conception, stated in Aristotelian terms, is therefore inadequate.

An organismic conception, as opposed to an elementalistic one, requires a non-Aristotelian logic not only because the relativity of the subjects of predication ('substances') invalidates the law of identity; such a view is also non-Aristotelian because it demands the rejection of the Aristotelian law of excluded middle (or principle of *tertium non datur*). Lewin's principle that $B = f(P, E)$ is accepted in the non-

Aristotelian system, but we recognize that any statement at best only approximates absolute certainty as a limit. In non-Aristotelian logic the traditional two-valued judgments of 'truth' and 'falsity' are replaced by an *n*-valued logic of probability (modality). Truth becomes a limiting case of a multi-valued logic, which limit is seldom attained because 'laws' and even 'facts' are abstractions from idealized situations. In biological work, for example, no two records of the smoked drum of a kymograph are ever absolutely alike; they are only more or less 'similar.'

We have suggested that the earlier history of physical science should enable us to extrapolate the subsequent history of psychology as it passes from its Galilean to its post-Galilean phase—or what we here would term its non-Aristotelian stage. Now if physics is to be our guide, we may expect the future of psychology to exemplify the outlook of present-day relativity physics, insofar as this now represents a stabilized and acceptable doctrine. Here again, in transposing generalizations from one field to another, it becomes obvious that if Gestalt theory were more alert to the possibilities of the non-Aristotelian developments it might avoid some mistakes it may otherwise have to make. One illustration of this must suffice.

The weakness of classical physics was the necessity for absolutes. Space, time, mass, force, etc., were hypostatized and made into things-in-themselves (absolutes), not only because of the influence of the subject-predicate logic, but because of the presuppositions of the anthropocentric and geocentric cosmology carried over from Aristotle through Ptolemy into the views of Galileo and Newton. Today relativity physics has eliminated the privileged frame of physical science, and all the foregoing 'absolutes' have been relativized. But Gestalt theory, freeing itself from some of these traditional presuppositions, still preserves the uniqueness of a privileged frame of reference—no longer the fixed frame of the earth, or a *primum mobile*, or the ether of traditional physics, but the privileged frame of an observer who splits space and time in the good old Newtonian fashion,

contrary to the teachings of contemporary non-Newtonian physics. To see this let us return for a moment to Aristotle.

The place of the doctrine of *forms* in Aristotelian metaphysics is well known, and it need not be emphasized that for Aristotle *forms* were absolute. Gestalt theory likewise seeks the principles underlying the realization of *forms* (*Gestalten*), though it aims to avoid the teleological implications of the Aristotelian cosmology. But in all fairness we ask: *Is there much difference between the declaration in the geocentric cosmology of Aristotle that planets move around the earth in circles because the circle is the perfect geometrical figure and everything strives for perfection, and the Gestalt law of 'Prägnanz' as symbolizing the realization of forms in nature?* And is it not apparent that the Gestalters here have ignored the teaching of relativity physics that 'forms' are relative, so that, *e.g.*, what is a straight line in one frame of reference is a curve in another and a circle is (or becomes) an ellipse when viewed from a frame moving at right angles to the 'circle'? In brief, does not Gestalt theory, which, in seeking to assimilate life and nature (as Koffka says), renders lip service to physics as a fundamental science, nullify its own program by ignoring present-day non-Newtonian physics through its virtual reintroduction of the 'local' time of the privileged coördinate system of the psychological observer? Gestalt psychology starts from the ego-frame of the observer and proceeds to study the laws governing the formation of spatial and temporal wholes (Gestalten), but if we are to take our physicizing seriously, at least to the extent that we seek the isomorphic relations between phenomenal, physiological and physical structures, should we not recognize that every Gestalt is a four-dimensional space-time organization? It may be that a psychology that attempts to assimilate life and nature will find it necessary to modify or supplement relativity theory in some way, for as Émile Meyerson has pointed out, the irreversibility of experienced time has no correlate in the symmetrical (reversible) space-time manifold of relativity.

Up to this point our treatment has largely been negative, pointing out what, from our point of view, is wrong with pres-

ent-day Gestalt theory. In order to map out a positive program for the future which may embody the organismic (non-elementalistic) conceptions here asserted to underlie the non-Aristotelian theory, it is necessary first of all to correct the incompleteness of Lewin's theory. This theory assumes that there are only *two* modes of thinking, but as a matter of fact there are really *three* modes of orientation, or 'semantic reactions' as Alfred Korzybski calls them. These three are as follows: (1) the *pre-Aristotelian mode of orientation*, as revealed by Lévy-Bruhl's studies of primitive mentality; (2) the *Aristotelian mode of thinking;* (3) and the *non-Aristotelian adjustment*, including here the post-Aristotelian or Galilean type of thinking of Lewin. I call attention to the omission of the first type because the third type seems to resemble in some respects a return to the first type of orientation to nature. To demonstrate this let us turn for a moment to Lévy-Bruhl's researches.

For over a quarter of a century Professor Lévy-Bruhl has expounded the thesis that primitive mentality is 'pre-logical,' in the sense that it does not conform to the categories which the scientific reasoning of modern culture has established. His anthropological investigations have shown, he believes, that 'primitives' do not observe the fundamental canon of Aristotelian logic, the law of contradiction, but follow an entirely different principle which Lévy-Bruhl designates by the term *participation.*

The implications of this challenging view are so far-reaching for social science not only because they invalidate much of accepted anthropology, but also because such implications lead us to the conclusion that the 'laws' of our thinking and explanation are a product of biological and social evolution. This in turn suggests to logic and psychology the possibility of still further change along the lines of mental development for the race. In proposing that the third stage in this evolution is, or will be, the non-Aristotelian mode of thinking, and in suggesting that this new level of explanation will yield its own insight into the unity of nature—an insight in some respects paralleling primitive man's 'apperceptive synthesis'

—we are of course merely indicating our own theory of the way in which future possibilities will be unfolded. I mean by this that after the present age of specialization in science has passed, or has been supplemented by an era of coördination and synthesis of knowledge, we may again attain an understanding of the interconnectedness of things which resembles primitive man's sense of 'participation,' in the sense that here, on a higher level, we again realize the limitations of the classical laws of thought.

In support of this unorthodox hypothesis recall that the common element of the Galilean mode of thinking, as Lewin describes it, and the non-Aristotelian system, which is the last of our three levels, consists in the fact that in both schemes the study of the behavior of any 'part' and the explanation of the 'facts' is to be sought not merely in and through the nature of the thing itself, but in the whole of which it is a part. For us, however, 'wholes' and 'parts' are to some extent relative. We hold, moreover, that future progress in science will reveal new types of interrelation between relatively individuated wholes. This makes it clear that the explanation of any part is to be found in the mutual influences of the other parts of the wider universe. Thus we agree with Dr. A. N. Whitehead that the entire cosmos constitutes the environment of each empirical thing; but since each part is an abstraction and only relatively individuated, we assert—unlike Dr. Whitehead—that the so-called 'laws of thought' are only partially adequate to an understanding of nature.

In order to be more specific, we shall cite several examples of this new mode of understanding. First, however, we restate the three modes of orientation in terms of the 'axioms' on which they are based. On the first level, the *pre-Aristotelian* mode of adjustment of primitive man, the axiom is, *everything is everything else*. The 'animistic' system is an expression of 'mystical participation' in the sense that it does not distinguish between the *self* and *not-self*. There are no sharp dichotomies in nature because the 'laws' of *identity*, *contradiction* and *excluded middle* are not observed. On the next level of mental-social evolution, that of *Aristotelian*

logic, we get these sharp distinctions. Here the axiom is *this is this*, and *that is that*, and *this is not that*. But on the third level we return to the idea that *everything is everything else*, except that this non-Aristotelian principle is based on a deeper understanding of the unity and interrelatedness of nature than was primitive man's response.

Now in looking for instances of this new insight we might well turn first of all to the foundational science of physics. Here we find Sir Arthur Eddington telling us that wave mechanics provides one meeting point for relativity theory and quantum theory (**2**, p. 108). He states that the electron as such has no physical properties. If it were absolutely alone nothing whatever could be said about it (**2**, p. 225). In present-day physics the most fundamental equation is the wave equation of an electron, which gives a relation between the size of the electron and the universe. That is to say, the wave equation describes the electron in relation to a physical comparison object, and for Eddington this is the universe as a whole, the Lemaître spherical universe. This, of course, is a further development of Einstein's relativity theory in which mass is a manifestation of the curvature of space-time. It also turns out on this theory that the number of particles in the universe has not been decided by arbitrary choice, but is fixed by an inner necessity, the cosmical constant (p. 253). Thus one may find relations in nature which tell the behavior of the universe in terms of an electron and to measure the mass of the electron one can make observations of the distance and velocity of spiral nebulæ. This, I submit, is really something new in thinking!

When, several years ago, the writer first presented the idea that modern physics requires a non-Aristotelian logic, because of the fact that the law of identity is rendered not-true by present quantum data (of the wave-particle opposition), several critics of the author vigorously objected. Specifically, Dr. Herbert Feigl pointed out that P. A. M. Dirac's theory of radiation promises successfully to resolve the wave-particle difficulty in physics. But in our subsequent volume, *Philosophy and the Concepts of Modern Science* (**9**, Ch. 3), the writer

reaffirmed the original position and pointed out that even Dirac's theory has not completely abolished this troublesome problem. It is significant that in the last edition of *The Principles of Quantum Mechanics* (1), Dirac admits in conclusion that "It seems that some essentially new physical ideas are here needed" (1, p. 297). That something like a non-Aristotelian principle is creeping in even in Dirac's theory appears in his interpretation of the *principle of superposition*, which affirms that between the states of an atomic system "there exists a peculiar relationship such that whenever the system is definitely in one state we can consider it as being partly in each of two or more other states. The original state must be regarded as a result of a kind of *superposition* of two or more states, in a way that cannot be conceived on classical ideas" (1, p. 12).

On the system of Aristotelian logic the proposition, "this system is in one state," would be true (or false), and the contradictory proposition, "this system is in another state," would be false (or true). But if, like Dirac, we say that the 'same' system is simultaneously in two different states, this violates the 'laws' of Aristotelian logic. Nevertheless, as the writer points out (8), this is exactly the non-Aristotelian situation we face when we say, *e.g.*, in neurology that the two propositions, "this neurone is active," and, "this neurone is inactive," are both true, for the evidence indicates that the same neural elements may be both active and passive at the same time. The way in which such 'paradoxes' are resolved lies in recognizing that the subject of predication is not an absolute, but, like a chemical solution, may be both electropositive and electronegative, according to the context (environment) it is in. Another way of bringing out this fact that quantum mechanics (like organism-as-a-whole situations generally) requires a non-Aristotelian logic is to assert that both the wave and particle pictures are *possible*, if we here regard this as one modal function of a multi-valued logic. In either case the rejection of the law of excluded middle is still tied up with the rejection of the fallacy of the *absolute individuality of the subject.* If we adopt the terminology of

George H. Mead, we might say that the wave character by means of which the individuality of the particle dissolves into the 'mist' of the indeterminate (of the 'probability wave') is an expression of the 'sociality' of the electron.[1] Thus the modernized Hegelian concept of emergence through the conflicts of opposites can be applied to all levels of nature, from the electrodynamic concept of matter to the dialectic of social development as Marx interpreted it. Eventually the defender of dialectical materialism will come to accept non-Aristotelian logic as a valuable instrument and ally.

With this general view as a background, one may proceed to the task of rewriting the entire domain of science in terms of such principles of *complementarity* and *relativity*. Along these lines, for example, the present writer has started from the supposition that the human time sense is a function of the velocity constants of the chemical reactions in the brain (3), added to this the idea of Dr. W. M. Malisoff that the velocity constants of chemical reactions are related to the earth's gravitational constant, and ended up with the idea that possibly the human time sense is a function of the rate of expansion of our 'cosmic bubble.' Aside from such highly speculative matters, it is certainly clear that such a way of viewing nature will eventually lead to a new theory of biological evolution, as indeed the writer has already proposed (6).

Since the time of Charles Darwin psychologists have paid their respects to the doctrine of evolution, but they seem not

[1] It is a very interesting fact that the conception of 'emergence' is finding support in the doctrine of relativity concerning the non-additive character of the velocity of light. This idea, set forth in different forms by George Humphrey and Alfred Korzybski, has received support of a somewhat different sort in the important conception of nature advanced by George H. Mead in his book, *The Philosophy of the Present* (5), especially in the chapter on 'Emergence and Identity.' Starting with the thesis that temporal transition is itself a kind of relativity and that sociability is a capacity for being several things at once, due to the fact that the novel event is in both the old order and the new order which its advent heralds, Dr. Mead arrived at an original interpretation of physical relativity. Here the increase in mass, which on relativity theory accompanies the increase in velocity, is due to the fact that the 'emergent' motion changes the physical character of its object (its mass). In a similar way emergent life changes the character of the world, just as emergent velocities change the character of masses.

to have realized that evolution is not yet done with the human organism. Both Aristotle and Darwin set the compass of human thought and marked out the boundaries of its explorations. But now we are beginning to see the limitations of their picture of man. A theory which goes beyond these great pioneers must build upon the foundations which they laid, but at the same time we must not forget that the mind of man cannot forever be tied to the past out of which it has emerged. Even now, we believe, the results of science call for and are helping to create a new, non-Aristotelian type of mentality. And in the future humanity as a whole will acquire different 'habits of thought.' Thus the evolution of the mind is a never-ending process.

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THE SPECIFICITY OF CORRELATIONS BETWEEN INITIAL AND FINAL ABILITIES IN LEARNING

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I

Reports on the size of the correlation coefficients that have been obtained between initial and final abilities in learning have varied tremendously. About the only generalization that one may make concerning them is that they are almost invariably positive, but they have been reported to be as high as .96 (3) and as low as .08 (10). The marked variability of the obtained coefficients suggests that they are a reflection of a differential set of conditions. It should be clear that while the correlation coefficient may very well be a function of many different aspects of the learning task and the learning situation it represents only an end product. Its numerical value alone cannot give any information which will be diagnostic of the relationships which determine its size. Before general statements as to the relationship between initial and final abilities can be made, it is necessary to examine the various experiments reported in which such correlations have been obtained in order to see whether the influence of specific factors can be determined. Further, it may be questioned whether such correlations, which may vary as a function of a number of conditions, yield information that is significant for the problems of learning, or even for the problem of individual differences.

In measuring the relationship between initial and final abilities in learning, we must obtain some estimate of the individual's standing with reference to the group at the beginning of a learning task and at the end. Ambiguity is immediately introduced by the concepts beginning and end. The true beginning would be represented by the condition of

zero ability for each subject. This condition would of course make a correlation technique inapplicable since some variability within the group is an absolute necessity for correlation. In actual practice, zero ability at the very start is not the case, but on the other hand equivalent practice for each individual in the function tested is not usual either. In correlating related tasks, or trials within the same task, higher values are usually obtained among the trials at advanced stages of practice than at earlier stages. McNemar (15) reports higher correlations among a series of mechanical tasks after practice than before practice and Hertzman (9) has obtained similar results in comparing the correlations among the later trials of a learning task with the intercorrelations of the earlier trials. These results indicate that when uncontrolled practice effects are made relatively less significant the correlations among practice trials will be altered.

The end of a task unless it is the solution of a problem is also an arbitrary matter since it may represent different degrees of development for different individuals. The organization at the end selected by the experimenter might not be the same organization that would be found if more trials had been added. An important consideration then would have to be the length of the task that was utilized and the amount of work sampled as a measure of initial and final abilities. In other words, the terms 'initial' and 'final' could not have identical meanings in different experiments.

Instead of stating the problem in terms of finding the relationship between initial and final abilities in learning, the problem at best becomes an analysis of the conditions which affect the relationship and the way in which they affect it. An examination of the various studies in this field indicates the existence of a number of conditions with respect to which there are differences. These conditions must be analyzed in order to see what they contribute to the relationship between initial and final abilities in learning.

The important differences that are found among the studies reported are:

(1) *Differences in the number of subjects used in each study.*

Many of the studies reported have been based upon very few subjects. Among the correlations reported below, we find that 28 out of 50 presented have been based on 22 cases or fewer. This condition makes for tremendous variability. The mean of a large number of correlations based on a few cases each would in all probability be close to the mean of those based on a large number of cases. Any one correlation, however, based on a few cases would less probably represent the general tendency than would correlations based on a large number of cases. Garrett and Anastasi (7) have demonstrated that when the correlation between two variables is known, the deviations of correlations obtained from empirical data from the true correlation decrease as cases are added. The variability of the correlations obtained in these learning studies could then be due to errors in sampling.

(2) *Differences in the length of the task.*

The learning tasks used have differed widely from each other with respect to length. While it is not easy to compare tasks of different materials and different time intervals per trial, a rough index can be obtained by classifying the tasks according to the number of trials permitted.

(3) *Differences in the length of time for each trial.*

A longer trial would in most cases enable individuals to be placed more accurately in relative ability than they would be in a shorter trial, and probably the use of longer trials would lead to higher correlations between early and later scores. But what may be considered to be a short trial and what may be a long trial is apt to be dependent partly on the nature of the task and the form of the learning curve for the task. Despite these considerations, we have no definite criteria which will tell us that a 10 minute trial for typewriting, let us say, is functionally shorter than a 2 minute trial in a symbol substitution test. In actual practice, we have to depend on the existence of a large sampling of tests to draw any conclusion about the time factor. These considerations apply to

tasks employing the time limit method. In tasks using the amount limit method, the problem is somewhat different, the variable being the amount of work sampled. In the analysis below, it was impossible to find criteria for the division of the amount limit tasks into groups of different sized trials. But comparisons between tasks employing the amount limit method and the time limit method have been made.

(4) *Differences in the nature of the task.*

The general nature of the correlation may differ in tasks involving different kinds of materials or functions. The correlation could be influenced by tasks which showed a variety of learning curves for the individuals measured, and by tasks which would permit the operation of different degrees of insight at various levels of the practice curve.

Several studies have been collected which report correlations between initial and final abilities in learning. Most of them contain data on more than one task or more than one group of subjects. From the 13 studies there are in all 50 correlations. The studies utilized are those of Anastasi (1), Chapman (3), De Weerdt (4), Hollingworth (10), Kincaid (11), Myers (16), Perl (17), Thorndike (19, 20, 21), Thorndike, McCall, and Chapman (22), Whitely (24), and Woodrow (25).

The major dimensions used by us for the classification of the studies reported by the above investigators were the number of subjects, the number of trials, and the amount of time spent per trial in the time limit tasks. In addition, an attempt was made to classify the tasks according to their nature, and comparisons were made between tasks employing the amount limit method as contrasted to the time limit method. Only a two-fold division was utilized for each of the three major dimensions. An attempt was made to place 50 per cent of the tasks in each division, though limitations of sampling did not make this quite possible. However, more than 40 per cent and less than 60 per cent of the available correlations are to be found in each division. The dividing line for number of subjects was a sampling of 22. Those studies having fewer than 22 subjects belong to the small

sample group. Those above are in the large sample group. The range of subjects for the former is from 7 to 22, for the latter from 28 to 200. In similar fashion, the boundary for trials was 19, with the short series ranging from 7 trials to 19, while the long series ranged from 20 to 205 trials. The time factor divided the groups into two varying from one-half minute to 2 minutes, and from 3 minutes to 20 respectively.

The median correlation and the interquartile range of the groups mentioned above, plus these measures for other combinations dependent upon the dichotomies described, are presented in Table 1. The column of Table 1 named the

TABLE 1

ANALYSIS OF THE INFLUENCE OF THE LENGTH OF TASK, THE NUMBER OF CASES, AND THE TIME PER TRIAL ON THE CORRELATION BETWEEN INITIAL AND FINAL ABILITIES IN LEARNING

Classification	Number of experiments	Median correlation	Interquartile range	Corrected interquartile range
1. Small no. of cases	28	.59	.53	.78
2. Large no. of cases	22	.61	.24	.42
3. Short series	26	.71	.25	.50
4. Long series	24	.42	.39	.47
5. Short series—small no. of cases	9	.75	.27	.63
6. Short series—large no. of cases	17	.68	.19	.32
7. Long series—small no. of cases	19	.50	.52	.66
8. Long series—large no. of cases	5	.39	.17	.19
9. Short time per trial	16	.53	.38	.49
10. Long time per trial	14	.71	.25	.50
11. Short time—short series	8	.63	.32	.50
12. Long time—short series	11	.76	.20	.45
13. Amount limit—short series	7	.68	.19	.31
14. Amount limit—long series	13	.50	.43	.57
15. Time limit—short series	19	.74	.27	.56
16. Time limit—long series	11	.24	.26	.29

'corrected interquartile range' was felt to be a necessary one particularly in comparing those groups whose median correlations were at different levels. The interquartile ranges are found by subtracting one correlation, the lower quartile, from another correlation, the upper quartile. Ordinarily obtained correlations cannot be subtracted from each other, since the

predictability of correlations at different levels are not related to each other in linear fashion. The upper and lower quartiles were accordingly transmuted into Fisher's 'z' (5) and the lower quartile was then subtracted from the upper to obtain the interquartile range. In this way variability estimates at different levels of correlation could be measured in comparable terms.

With a fairly large sampling of tasks, we find as expected that median correlations of the small subject and the large subject groups do not differ from each other to any great degree, the medians being respectively .59 and .61. But the measures of variability differ greatly from each other indicating that the wide variability of the obtained correlations are definitely dependent in part on differences in the number of individuals sampled in each study. For the two groups the corrected interquartile ranges are .78 and .42 'z' units.

The short and long series differ with respect to their central tendencies, the medians being .71 and .42 respectively. The critical ratio [1] of this difference (when the difference is in terms of 'z' units) is 5.1. The corrected interquartile ranges are about equivalent to each other. There can be no question as to the influence of the size of the task on the correlation. The longer tasks lower the correlation.

Similar results are obtained by an analysis of the various combinations that can be made of the tasks when both the number of cases and the number of trials are considered to be variables. The short series, regardless of the number of cases, has higher correlations than the long series, and the groupings containing the experiments with the small number of cases have the higher variabilities regardless of the number of trials.

The problem in regard to the influence of the time of the trials does not yield clear-cut results. The short time experiments yield a median correlation of .53 against .71 for the long time series. But we have already seen that if we are basing our results on a fairly sizable number of experi-

[1] The critical ratio was obtained by transmuting the medians into 'z' correlations. The difference was divided by the probable error of the difference which was obtained from the quartile deviations in the manner indicated by Garrett (6, formula 32).

ments the number of trials in the tasks will make a difference. An analysis of the data shows that 11 out of the 14 (79 per cent) long time tasks are short with respect to the number of trials, whereas only 8 of the 16 (50 per cent) short time tasks have a small number of trials. This unequal sampling operates to widen the difference, because of the higher correlations to be found in the short series. Therefore the two time periods were compared with reference to the short series.[2] The medians become .63 and .76 with a critical ratio of 1.8 (obtained in the manner described in footnote 1). There is a possible influence due to the time spent on each trial but our data do not prove the existence of such an influence conclusively.

The tasks assembled consist of 30 in which the time limit method was employed, and 20 in which the amount limit method was used. Since it is clear that the number of trials influences the size of the correlation, each set has been divided into long and short series. The obtained results are not very consistent. The short series for each kind of task have about the same median. In the long series the amount limit tasks have a higher median. The ratio of the difference between the two medians to its probable error is 2.8. The results are not clear-cut or consistent, and might very well be due to sampling errors.

A complete analysis of the median correlation for each kind of task used as well as the medians for the short and long series with reference to each kind of task is presented in Table 2. Unfortunately only 7 of the 16 different tasks that were found in the 50 experiments have been used in both long and short series. In all 7 instances, the median correlation of the short series is greater than the median correlation of the corresponding long series, a finding which verifies the results based on the gross totals. The correlation between the medians of the long and short series is .79. This figure would indicate that in this sampling of tasks there might be a correlation between initial and final abilities that is a function of the

[2] There were too few experiments of the long time, large number of trials type to warrant analysis of the effects of the time factor on the long series group.

TABLE 2

ANALYSIS OF THE INFLUENCE OF SHORT AND LONG SERIES ON THE CORRELATION BETWEEN INITIAL AND FINAL ABILITIES FOR A VARIETY OF TASKS

Task	Total		Short series		Long series	
	No. of exper.	Median corr.	No. of exper.	Median corr.	No. of exper.	Median corr.
1. Cancellation	8	.71	3	.79	5	.63
2. Addition	7	.74	5	.76	2	.37
3. Vocabulary	7	.56	5	.59	2	.24
4. Multiplication	6	.70	4	.78	2	.44
5. Card sorting	4	.41	2	.47	2	.39
6. Substitution	4	.40	3	.43	1	.25
7. Color naming	2	.78	1	.87	1	.68
8. Typewriting					3	.18
9. Making gates			2	.53		
10. Reading			1	.93		
11. Braille writing					1	.28
12. Coordination					1	.52
13. Dart throwing					1	.44
14. Discrimination					1	.67
15. Maze tracing					1	.15
16. Tapping					1	.23

particular task involved and is independent of the length of the task. Among the tasks for which there are both long and short representatives, the higher correlations are to be found among the cancellation, addition, multiplication, and color-naming tasks. The last-named task is represented by only one case in each series and each of the two experiments has been performed on a small number of subjects. The low correlations involve card-sorting, substitution tasks, and vocabulary. The vocabulary tasks are a heterogeneous group and perhaps were not rightfully merged in a common classification. Most of the tasks in each group are largely of a rote nature. All in all, no general conclusion seems feasible from these data about the influence of the nature of the task on the obtained correlation between initial and final abilities.

II

The question must be raised as to whether correlations between initial and final abilities add anything to our knowledge of the problems of learning. While it may appear that the question of the relation of original standing to later

standing in a learning task is significant for the problems of individual differences, we have found in the foregoing analysis that any meaning we may attribute to such correlations must be clarified by reference to some of the aspects of the conditions of learning. Even under the conditions most favorable for obtaining high and stable correlations, *i.e.*, the short series group of experiments based on a large number of subjects, we find that the median correlation of .68 is none too high for predictive purposes, and that the variability suggests the possibility of still other factors being influential in establishing the relationship. A correlation of .68 implies a predictability above chance equal to 27 per cent. The range of correlations in the above mentioned grouping was from .37 to .93, or a range in predictability of from 7 per cent to 63 per cent.[3]

We may conclude that the intra-task correlations do not provide a general solution of the problem of the relationship between the standing of individuals in an early part of a learning task as compared to a later one. We do not find that the correlations concentrate on a given level. On the contrary, we find them to be variable, their variability being a function of many conditions. This alone precludes any generalization about the nature of the relationship. It would therefore seem improbable that such correlations would answer the question of the relationship between early and later standing in a learning task. To put it another way, we might say that the analysis of this relationship is not legitimately a general question. Once the correlation has been obtained for a particular task under a particular set of conditions, it explains very little and most certainly the relationship it indicates cannot be taken as being a general or usual one. On the contrary, any obtained correlation must be understood in terms of the learning process. There are in addition many learning tasks for which the correlation could not be obtained.

When individuals differ in the way in which they learn the same task, or when there are tasks in which insight enters

[3] Predictability above chance is determined by subtracting the coefficient of alienation from unity (8, p. 363).

at one or more stages, there are conditions which call definitely for analysis of the individual modes of approach for the complete understanding of what has taken place. Even in tasks which are largely rote in character there are instances of different forms of curves for the progress of the individual over the interval studied. Thus in Asch's (2) study we find in three tasks apparently of a rote nature—symbol-digit, dot cancellation, and cancellation of A's—that while the individual curves are predominantly of the hyperbolic form there are a significant number of straight line functions. The hyperbolic curve in these tasks occurs 43 times against 13 occurrences of the straight line. The occurrence of different modes of progression would have influenced the relationship between initial and final abilities but the nature of the influence would in no way have been illuminated by reference to the absolute value of the correlation coefficient. Moreover, the size of the coefficient would definitely be affected by the point on the curve that the experimenter would choose to represent final ability. This contention becomes clearer by a specific illustration. If we imagine two individuals whose initial abilities are identical but one progresses along a curve of negative acceleration (hyperbolic curve) and the other with no acceleration (straight line), the relationship between the relative final positions of the two individuals may vary depending on the stage which was selected as being final. For the early trials, the individual whose learning was described by a straight line might be below the other. As the trials progressed they would get closer until the former individual reached and then passed the latter.

The mode of performance of the same task has been shown many times to differ from individual to individual. Different ways of performing may lead to great variations in efficiency as measured by the elimination of error or the amount of effort necessary to master a task. Yet such wide quantitative differences might be due to the operation of temporary sets or certain influences of past experiences which predisposed individuals to apply the methods they did use. The order of relationships at the beginning and the end of the tasks

might very well be different if the same mode of behavior were used by all the subjects. Warden (23) found that individuals utilizing the word reaction method of learning the stylus maze did so well as a group that the most inefficient individual in this classification was superior to the most efficient member of the contrasted motor reaction group, with very large differences existing between the averages of the groups. Although the members of the word reaction group were superior on the average to the motor group on the intelligence tests administered, the differences were not very marked and the large differences obtained with the learning data must be attributed largely to differences in method of attack. Here we find wide quantitative differences as a result of what appears to be a qualitative difference in approach. If all subjects were briefly instructed in the word reaction method, large changes in the course of learning would have taken place in those individuals who did not utilize the method originally. If some measure of relative standing in two stages of the learning process had been obtained, this measure would probably have been altered when all were using the same method. Again the correlation would be of no particular value in understanding the process itself. Similarly if the original performance of the individual were organized in accordance with some plan which was not conducive to solving the problem we should have to investigate the way in which the performance shifted and why it shifted. Thus Krechevsky (12, 13) presents us with excellent techniques for analyzing discrimination data in terms of methods of attack which illuminate the nature of the errors obtained.

When faced witht he problem of all-or-none learning, correlations between initial and final abilities are not obtainable. In the study of Kreezer and Dallenbach (14), children who understood the concept of 'opposite' were correct in practically all of their answers; children who did not were incorrect practically all the time. There was no real continuum. In more complicated tasks, such as Ruger's (18) puzzles, there may be several different features which are learned in an all-or-none manner and even in mazes op-

portunities for all-or-none and insightful learning may be present. Various delays in completing a task may be due to the fact that there is no learning going on at all. With temporary sets operating in the all-or-none and insightful kinds of tasks, initial and final abilities correlations take on less and less meaning even when obtainable.

In conclusion we may say that any correlation between initial and final abilities tends to be a statement of a specific relationship which is dependent on a variety of conditions. We therefore cannot speak of the existence of a general relationship between initial and final abilities in learning. Whatever relationship we find does not increase our knowledge of the learning process. On the contrary, the relationship must be understood in terms of the learning process and the conditions of learning. The correlation becomes less and less meaningful as instances are found of individuals whose performance is organized in entirely different ways, and it cannot even be obtained in tasks consisting of a single all-or-none unit.

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ON SOLUTION-ACHIEVEMENT

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Experimental investigations on learning and thinking have usually been conducted as though they belonged to two separate fields of inquiry. The assumed difference between these two fields has rarely been explicitly analyzed or stated but has existed largely as a sort of gentleman's agreement among experimental psychologists. During the past year the writers have had occasion to discuss questions which inevitably seemed to involve both fields. It soon became clear that if some unifying schema could be arrived at, many pseudo-differences would be avoided and a single, comprehensive analysis detecting real likenesses as well as differences would be possible.[1] We therefore thought it worth while to propose such a schema and analysis for further discussion. It should be made clear, however, that our present concern is a predominantly formal one. Only in a minor measure are we here concerned with the 'dynamics' of learning or thinking. We feel that for purposes of further study the 'anatomy' of a subject-matter might well precede a clear statement of its 'physiology.'

[1] As far as the writers are aware the only other explicit differentiation which has been attempted between 'learning' and 'thinking' is the one made by Maier. Maier (5) differentiates between a 'reasoning ability' and a 'learning ability.' The former involves, according to Maier, the ability to combine two isolated experiences, and the latter, the ability to combine two contiguous experiences. Rather than differentiating between 'abilities' the present analysis attempts to differentiate between processes in such a manner as not to draw a sharp dichotomy between the two, but to indicate the inter-relationships existing. Further, according to the present analysis, Maier's two abilities would both be subsumed under the more general process of learning.

Most psychologists would agree that all learning involves two processes. First, something must be *acquired*, and secondly, something must be *retained* to some degree. For methodological as well as historical reasons, experimentalists have tended to investigate these processes separately and to assign to these processes different degrees of importance for the general question of learning. One group, taking the process of acquisition for granted,[2] concerned itself primarily with the field of 'memory,' studying such factors of retentivity as time, number of repetitions, the nature of the material, the method of presentation, the intervening activity between training and recall, physiological conditions of the subject, etc. The second group, taking the process of retention for granted, concerned itself with the investigations of the *modes* of acquisition, the manner in which the subject 'arrived at' the material which was to be retained. These experimenters studied such processes as conditioned response, trial-and-error, abstraction by varying concomitants, application of previous experience, insight, etc.[3]

One of us (Krechevsky) has worked primarily in this latter field of learning—modes of acquisition—while the other (Duncker) has worked in the 'different' field of thinking.[4] In discussing our separate experimental work it soon became obvious that we were both using very similar concepts. This similarity pointed to a close relationship between the two fields, which we have come to formulate in the following way: Learning involves acquisition and retention. Of the many types of acquisition possible in various learning situations, the type which occurs in problem-solving (involving the *discovery* of a solution by the subject) is called 'thinking.'[5]

[2] These investigators worked in fields where *acquisition* did not involve much more than the understanding of instructions and the attentive observation of 'readymade' material.

[3] This distinction between acquisition and retention—a distinction similar to the one made by Koffka—is no absolute one. Acquisition itself requires a certain span of immediate retention, and retention is often a sort of prolonged acquisition, inasmuch as the material is continually undergoing qualitative changes.

[4] 'Thinking' in the functional sense of problem-solving, not in the material sense of a special, *e.g.*, imageless, kind of representation.

[5] As contrasted with the subject's being told, or looking up the solution, or becoming conditioned to the solution presented by the experimental situation, etc.

Thus learning, according to this simple formulation, becomes the more comprehensive concept, first because 'thinking' is only one type of acquisition, and secondly because learning involves the additional process of retention.[6]

On the basis of the above analysis it becomes comprehensible why the writers, working in the two fields of learning and thinking, could nevertheless arrive at similar concepts. Krechevsky, in his learning experiments, has been concerned with the process of acquisition in situations where the animal is confronted with a *problem* the solution of which it has to discover—which means that those experiments dealt with the same general type of situation which characterizes Duncker's experiments on 'productive thinking' in human beings. Further, both have been primarily interested in the 'qualitative' aspects of problem solving, *i.e.*, in the progressive modes and changes of the subject's procedure rather than in the efficiency of his response as expressed in terms of time, trial and error units.

The most general point in which our findings coincided was this: *Between the moment the organism is confronted with the problem and the moment the final solution is achieved there occur, as a rule, a number of intermediate steps leading, in an hierarchical fashion, from general to more specific features of the sought-after solution.* It might be well to outline the original two analyses separately and thereafter make a more detailed comparison.

In analyzing Krechevsky's experimental data on the problem-solving behavior of the rat in the discrimination-box, Tolman and Krechevsky (7) suggested the following schema: When the animal is first introduced to a problem situation he will exhibit a relatively large range of stimulus-response possibilities. This widest range was called a 'means-end-readiness.' But within any such general M-E-R there usually appear one or more specific alternative limitations.

[6] Here again we wish to emphasize that this suggested relationship between learning and thinking is not to be understood as implying that within the process of thinking learning plays no part. Most certainly thinking makes use of *previous learning*. Previous experience supplies much of the 'material' which then becomes 'reorganized' under the 'stress' of thinking.

Each of these limitations was called an 'hypothesis.' Learning was then phrased as consisting of the *reduction* of a more general M-E-R to some more specific hypothesis. It was further assumed that the acquisition process may involve a succession of such reductions, *i.e.*, from a M-E-R to an hypothesis and then from the latter, now called a M-E-R, to some still more specific hypothesis, and so on. It further follows that learning can also consist of the reverse process, *i.e.*, the giving up of a more specific hypothesis and *reverting* to a more general M-E-R and from there back to a different hypothesis. Once the correct hypothesis is achieved, other factors were assumed to step in. Thus, at first, the correct hypothesis (now called a 'docile habit') plays the role of an efficient means of achieving a given end in a given situation. As that situation is repeated, and consequently the 'docile habit' is exhibited more and more frequently, its nature changes until after sufficient repetitions the response-pattern loses its docile character and is exhibited even when it no longer serves the function of achieving a given goal efficiently. At this stage the response pattern is called a 'fixation' (Krechevsky and Honzik, 4). In terms of the analysis of learning here described, a 'fixation' is a response which has become so dominant that reversion to a more general M-E-R and thence a progression to a new and 'better' alternative hypothesis has become relatively difficult. Such fixations can also be due to emotional shock (Hamilton and Krechevsky, 3), impaired central nervous system (Hamilton and Ellis, 2) and other factors.

> Thus, to illustrate the above analysis concretely, the learning behavior of a rat in a brightness discrimination-box is described in the following manner: When first introduced into the discrimination-box the animal does not go to sleep, or sit and squeal, etc., but of all the possible behaviors exhibits a fairly definite pattern of behavior which we may term 'exploratory.' This general exploratory behavior is, in terms of the above analysis, the first means-end-readiness. But very soon, within that general M-E-R, the animal may begin to pay attention to the spatial

features of this to-be-explored environment, *e.g.*, he may begin to enter only every left alley, or only every right alley, etc. This specific response we now call an hypothesis. He may give up this hypothesis after a while and try others until he finally hits upon the correct one. At that point he is exhibiting a docile habit, for should the experimenter now reverse the significates of the cues and make the dark alley correct (assuming that in the original set-up the lighted alley was the correct one), the animal, after a few errors, would give up the light-going response relatively soon. On the other hand, if no such change is made and the animal is allowed to choose the lighted alley and is rewarded for so doing a sufficient number of times, the light-going response now becomes a fixation, for if *now* the significates of the light and dark cues should be reversed, the animal would find it extremely difficult to reverse his behavior appropriately.

According to Duncker's analysis of problem-solving (1) the first step to occur is usually of the nature of a general 'range' within which the solution is then sought for. Such a range can consist in either some general property of the sought-after solution (Chapter 1) or some general method by which to arrive at the solution (Chapter 2).

Within such a range there would then arise 'functional solutions' specifying the demands which the solution would eventually have to fulfill. These functional solutions have the typical form: "If such and such could be achieved the problem would be solved. How then to achieve it?" In other words a functional solution is of the nature of a specified problem. If a problem is defined as a relational pattern containing a lacuna, a functional solution as above defined is a solution in the sense that it describes the functional properties of the missing link, and is a problem inasmuch as it does not specify the missing link concretely. A functional solution arises either through making explicit certain significant implications of the goal (Chapter 2, Chapter 3), or by mobilizing suitable general experiences (Chapter 5). A total process of problem-solving may contain several 'generations' of functional solutions.

The final or 'specific solution' then comes as an *application* of a functional solution to the available data of the situation (supplemented by one's store of particular experiences). A solution is final if no more remains than the actual carrying out of the solution for the achievement of the goal.

If a given suggestion for a specific solution is found to be unsatisfactory, the subject reverts to the stage of one of the preceding functional solutions to seek some other specific solution, *i.e.*, one which will avoid the particular fault of the former attempt. These retrograde steps may be called 'regressions.' They lead to alternative and corrective solutions (Chapter 1).

Ranges and functional solutions may become fixated, inhibiting the adoption of other equally feasible ranges and functional solutions by once and for all profiling the situation into certain constant and variable features. Besides this 'fixation of approach' there is a 'fixation of context' (an element becomes less available for use if it 'belongs' to another context, Chapter 7), and then finally there is a 'fixation of structure' (an element 'resists' undergoing a contrary structuration, Chapter 8).

To illustrate the above analysis from an experimental case. The *problem:* To cure a man of a tumor in the stomach by applying certain rays which if sufficiently strong can destroy organic tissues. The problem then is to destroy the tumor without at the same time destroying the healthy tissues surrounding the tumor. *General range:* (1) This must be accomplished by physical means (rather than by speeches or prayers); (2) inquire into the *nature* of the conflict here presented, *i.e.*, what is wrong? *Functional solutions:* (1) Avoid contact with healthy tissues; (2) immunize the 'good' tissues so that the rays would do no harm to them; (3) by some means lessen the intensity of the rays as they pass through the good tissues. *Final solutions:* (1) Transmit the rays through the œsophagus (thus avoiding contact with the healthy tissues—functional solution 1); (2) inject some chemical into the healthy tissues (thus immunizing the good tissues—functional solution 2); (3) send several bundles of weak rays

from various points outside the body, all these weak rays converging at the point of the tumor (thus lessening the intensity of the rays at any given point in the periphery of the healthy tissues—functional solution 3). *Regressions:* A subject may have given as an attempted final solution that of cutting an opening through the body to the locus of the tumor. The quickly realized futility of such a suggestion may induce the subject to revert to the previous more general determination of seeking an open path, specified by the correction "it must be a *natural* open path." This may lead to the œsophagus solution. *Fixations:* The subject may never achieve final solution 3, because he will continue to try to solve the problem using but *one* bundle of rays, etc.[7]

It is obvious that both Duncker's analysis and Krechevsky's analysis have a great deal in common. Where Krechevsky and Tolman speak of means-end-readiness at its most generalized level, Duncker speaks of range; where in the former analysis, the term hypothesis is used, Duncker has the somewhat corresponding concept of specific solution. Both analyses describe the process of solution-achievement as involving an hierarchical succession of reductions or specifications. Both conceive of the reverse process and ascribe a similar role to it. Finally, both analyses consider the phenomenon of fixation and its inhibiting influence on problem-solving.

There is, however, in Duncker's analysis—besides such elaborations as are primarily due to the greater complexity of human problem-solving—one major concept which does not find a close parallel in the Tolman-Krechevsky analysis.

[7] At this point it may be desirable to say a few words about the relationship between Duncker's analysis of productive thinking and Maier's analysis of reasoning. What Maier's concept of 'direction' amounts to, seems to be an emphasis of the fact that a 'functional solution' has the directional properties of a (specified) problem. Maier's definition of reasoning as the combining of several separate experiences into one new response seems to refer to a basic feature of processes which involve successive steps. For example, the œsophagus suggestion is a new combination of two separate ideas: (1) Where there are no healthy tissues, no healthy tissues can be destroyed, and (2) within the œsophagus there are no healthy tissues. Therefore transmit the rays through the œsaphagus. This quasi-syllogistic procedure, in which Maier sees the essence of reasoning, is a special case of what Duncker has defined as 'insight.'

That is the concept of the functional solution. A closer consideration of this discrepancy will lead, we believe, to a justified theoretical differentiation between two common types of problem-situations. Apart from the intermediate position which it shares with a M-E-R and an hypothesis, a functional solution is characterized by the fact that it clearly defines certain conditions which, if fulfilled by the get-to-be-discovered specific solution, could not but resolve the problem. This one aspect of the functional solution does differentiate it both from the M-E-R and the hypothesis. The reason for this difference between the two, we believe, is due to the following factors: In Duncker's experiments the solution to the problem is a meaningful one. That is to say, the correctness or the wrongness of the solution is *not arbitrarily decreed by the experimenter but follows from general functional relationships of the world at large.* This means three things: (1) that the organism can bring to bear upon this particular situation all sorts of experiences which were made on other occasions; (2) that, owing to the generality of the basic causal relationships of our world, the organism is capable of tackling innumerable concrete instances by 'applying' comparatively few general experiences, and (3) that what there is in our world and in the problem situation of *truly necessary or intrinsic relationships* can, by some sort of insight,[8] be utilized for problem-solving.[9]

In the type of problems presented to the animals in Krechevsky's experiments (*i.e.*, sensory discriminations) the aforementioned relationships do not hold in the same manner —if at all. The discrimination problem is essentially an arbitrary one. Therefore, before the animal has actually tried out each specific solution or hypothesis he cannot possibly know whether the solution will work or not. Of course,

[8] We are here thinking, for example, of such geometrical relationships as permit inferential learning in the sense of Tolman and Honzik, or reasoning in the sense of Maier. For a further theoretical analysis of such insight the reader is referred to Duncker, Chapter 4.

[9] That these relationships, given the nature of our world, are truly necessary or intrinsic is a formulation suggested by only one of the present writers (Duncker) and does not represent the views of the other writer.

to some extent this holds true of all actual problems since there is always the possibility of interfering factors and quantitative miscalculations. Our point, however, is that what is *impossible* in arbitrary problems is at least *possible* in meaningful ones, *i.e.*, a well based conviction without actual verification.[10] This difference between arbitrary and meaningful problems is what makes a functional solution possible in the latter case and impossible in the former.

Summary

(1) A general relationship between learning and thinking is suggested to the effect (*a*) that learning consists of the two processes of achievement and retention and (*b*) that thinking is 'achievement' in working out solutions to problems.

(2) With the above schema in mind, two analyses, one derived from experimental work on thinking in human subjects, and the other from discrimination-learning experiments with rats, were compared and found to be strikingly similar. On the basis of that comparison, and stressing the points of coincidence, we would suggest that problem-solving behavior (both of the thinking type investigated by Duncker and of the discrimination-learning type investigated by Krechevsky) consists of a reduction of a general *range* or *means-end-readiness* to a more specific *functional solution* (in the former case)

[10] Hence, what Muenzinger and, following him, Tolman have called 'Vicarious Trials and Errors' (VTE) must needs play a different role in the two types of problem. VTE is defined as a characteristic behavior of rats at the point of choice between two stimuli. It consists in a 'looking or running back-and-forth-in-front-of.' We assume that this is in truth a vicarious form of trial and that the animal, when he is displaying this behavior, is being stimulated by the cues and, without actually trying them out, is trying to 'decide' (by some sort of 'reflection') whether or not they are likely to be right or wrong. Muenzinger, in a recent publication (6), has presented some extremely interesting data which clearly indicate a definite relationship between such VTE behavior and learning efficiency.

We now want to suggest that in the 'arbitrary' type of problem, VTE is useful only as a mode of *reviewing* and weighing evidences for further factual experimentation, or, as Muenzinger himself points out, VTE in such cases "has the function of aiding the re-instatement of the after-effects of past responses to the discriminanda of the situation" (Muenzinger, 6). The functional value of VTE in 'meaningful' problems, on the other hand, is essentially different. In these latter types of problems, VTE behavior *alone* may actually lead to the correct specific solution, without first engaging in actual trial-and-error behavior to obtain 'information.'

and *hypothesis* (in the latter), and then to still more *specific solutions* or specific hypotheses until the solution is achieved. Once achievement has occurred, an habitual performance of this solution may so change it as to fixate it and make it more difficult for the organism to change his behavior when such a change is demanded by his environment.

(3) Finally, a differentiation is made between two types of problem-situations—the meaningful and the arbitrary. The former contains *general* relationships, *i.e.*, relationships independent of, but applicable to the given situation, while the latter demands specific experiences with the specific problem-situation before such relationships can be known.

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STRUCTURE OR FUNCTION IN THE DEFINITION OF LEARNING?

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I. INTRODUCTION

Just what learning is and how it should be defined may appear to possess purely academic and rather tenuous implications. Yet there has lately arisen a distinct cleavage or dichotomy with reference to the definition of learning which tends to fog or confuse the meaning and interpretation of this term, and the scientific field it represents. In view of the great importance of the subject of learning to both pure and applied psychology, there is a real need for clarifying the issue. The object of this paper, therefore, is, first, to evaluate critically the two points of view which have been emphasized, and, second, to point to a new line of experimental evidence which bears directly upon the problem.

Professor Hulsey Cason has recently proposed that learning be defined as *the establishment or strengthening of neural connections between stimulating processes and responding processes as a result of accompanying or immediately preceding psychological activities* (3). The present writers have taken a different position, which is in opposition to that of Cason. It is Britt's view that learning is *the acquisition by a living organism of a mode of response which is adapted to a motivating problematical situation*[1] (1); while Kellogg has offered the following as a definition: *Learning is a persisting change or*

[1] The word 'problematical' in this statement has been used in a special sense to indicate 'some lack of adjustment between the organism's (*a*) motivating needs, (*b*) its reactive equipment, and (*c*) the sensory situation' (1, p. 468).

modification of behavior which results from repeated or continuous stimulation (15). The main point at issue is whether learning should be defined in terms of the nervous system (Cason) or whether it should not (Britt and Kellogg).

II. Structure Versus Function

The problem may, in fact, be regarded as a special aspect of the structure-function controversy. Is learning to be conceived in terms of structure (nervous system) or function (behavior)? That there exist two possibilities in this connection has been neatly emphasized by Woodworth (25, p. 224) who has given not one, but two, definitions of learning in the latest edition of his textbook of Psychology. They are (*a*) learning is *the process of developing structure through activity*, and (*b*) learning is *any activity of the individual which develops him and so affects his subsequent activity*. These definitions are very broad. They would even admit the increase in the strength and size of a muscle through exercise, as an elementary form of learning. Here, of course, the change in structure is unquestioned, for the altered size of the muscle can be seen and measured.

Now structure and function are often thought of as mutually dependent variables, somewhat like heredity and environment. It is commonly held by biologists that there can be no change in function without a corresponding change in structure; and conversely, that there can be no change in structure without a resulting change in function. In applying this principle to the field of learning, it must follow that here, as in other fields, structure and function are not to be taken as independent or separable one from the other. There is only this modifying or qualifying statement to be added to the particular application of the principle to the problem of learning—the behavior is usually overt and directly observable, but the structural change is obscure, and in many cases it is difficult, if not impossible, to demonstrate at all. Therefore, it seems to the present writers that the common tendency among psychologists has been to emphasize the function and 'soft-pedal' the structure. As a general thing

the learning has been taken to be the behavioral change, and the structural modification is regarded as its concomitant, or perhaps its 'cause.'

The relationship between structure and function in learning has been well expressed by Harlow (14) in the title of a recent article, *The neuro-physiological correlates of learning and intelligence.* Such a statement seems to label learning as the behavioral changes, while the neuro-physiological *correlates* are something like Cason's 'strengthened connections,' which may be behind the behavioral changes but are not *of themselves* the learning. The current operational movement in psychology lends special weight to this distinction, since operationism would favor a definition of learning in terms of the operations of measurement (22), and hence, by implication, in terms of something which can be readily measured, namely, behavior. Even in Woodworth's definitions, behavior (*i.e.*, 'activity') is given an important place. Neither of Woodworth's statements ignores behavior and defines learning exclusively in terms of structure. It should also be noted that the structural changes of Woodworth are not necessarily confined to the nervous system alone, but are admissible in other parts of the organism, perhaps in the effectors and receptors. In Cason's definition, however, the structural changes are specifically described as the establishment or strengthening of *neural* connections.

III. Objections to the Functional Viewpoint

Professor Cason (4, 5) has been good enough to offer welcome criticism of the definitions of learning proposed by each of the present writers, for which we thank him. His objections seem to be chiefly these.[2] First, that learning is really inside of the organism (structure), and that the behavior which we have stressed is just the symptom or evidence of what goes on inside. Therefore, we are confusing symp-

[2] There are several smaller points of disagreement. For example, Professor Cason says that "both Dr. Britt and I have used the words learning and memory as nouns . . ." (5, p. 441), although Britt had stated that this was the very position which he did *not* adopt (1).

toms with facts. Second, that the Britt and Kellogg definitions do not include some things which are generally agreed to be real learning, and that, on the other hand, they do include some other things which are generally agreed not to be learning at all.

'Symptoms' and facts.—The idea that the behavior is symptomatic of the basic neural connections which are behind it represents a position—as can be inferred from the previous discussion—which would be credited by the present writers with a strong *probability* of being true. There are, moreover, some interesting similarities between the symptoms of a disease and behavior modification in learning, which Professor Cason has pointed out (4). But the relationship between learning and medical diagnosis which Cason emphasizes can be thought of only as a rough analogy. For while the physician may diagnose a disease by observing the patient, the disease either has, or has not, an observable structural entity, depending upon whether it is (*a*) an organic or (*b*) a functional disorder.

Consider first a typical instance of an organic disturbance, for example, a case of cancer. External behavior may be symptomatic of cancer, but the structural change inducing the behavior can be surgically examined. It is a known, or at least a knowable, quantity. The behavioral change in learning, on the other hand, if it is really a symptom of structural *neural* modification, remains, as we shall presently show, the 'symptom' of so obscure and unobservable a change that, although probably real, it must be classified at the present time as little better than an assumption.

Consider next a case in which the disease is functional, as in the neuroses, or in certain types of heart ailment. Do such disabilities have any structural basis at all? If the structure-function reasoning of the biologist is applied to the answering of this question, then the answer is yes—since all functional changes must possess some structural foundation. But the very fact that disorders of this sort are described as 'functional' means, we take it, that their structural basis, if any, *has not yet been discovered or identified.* The individual sim-

ply 'behaves' abnormally. Because the structural changes are completely unobservable, the symptoms constitute the entire pathological picture. If now, Professor Cason would be willing to agree that learning, like a functional disease, is a phenomenon whose structural basis is as yet obscure and unidentified, he would have granted all that the present thesis demands.

Errors of inclusion and omission.—Definitions of learning, or of anything else, may be subject to errors of inclusion and errors of omission. A definition which commits the error of inclusion is so broad that it will admit items which do not rightfully belong in the category or field which it seeks to define. In Cason's opinion, the definition by Kellogg (a persisting change or modification in behavior which results from repeated or continuous stimulation) includes too much, for it can really be thought of as including growth or maturational processes, and growth, from Cason's viewpoint, should not be confused with learning (4).

Definitions, of course, may be broad or narrow, specific or general; and they may vary in many other respects as well. There is surely no such thing as 'one correct and perfect definition' of learning, in any absolute sense. All definitions are relative to the facts and purposes from which they have arisen. Thus, Washburne's definition of learning, 'an increase, through experience, of problem-solving ability' (24, p. 610), is adequate for his special purposes, although it may appear to be limited if generally applied.

Moreover, definitions in a science are for the most part tentative, and must be modified, or perhaps even discarded, with the discovery of new facts. As Stevens says in his discussion of operationism, "No concept can be defined once and for all" (23, p. 527). The value of a definition at any particular time must depend upon its usefulness and applicability at that time. One definition may prove more suitable for certain special purposes than another definition, and would then be preferred to others for the purposes for which it was fitted. However, if the purposes or objectives

should change, then the definition which was originally discarded as inadequate might become entirely acceptable.

With reference to growth and learning it has recently been pointed out by Carmichael that "changes resulting from environmentally released activity [*i.e.*, learning] are not necessarily physiologically unlike those resulting from the so-called inner processes of growth" (**2**, p. 467). On the basis of Carmichael's conclusion, we may then say there is no need for defining learning *in the broad sense* in a way which would exclude growth. If, however, one's objectives should demand a differentiation between growth or maturation, and the learning process, the present writers would then propose the following as a narrower and more precise statement: *Learning is a persisting change or modification of behavior which results from the activity of the organism itself, and which tends to adapt the organism to its environment.*

IV. Objections to the Structural Viewpoint

The second kind of error to which definitions can be subject is the error of omission. This means, with reference to definitions of learning, that there may be recognized instances of learning—generally agreed upon by psychologists at large—which a definition does *not* cover. We are forced to confess, after careful consideration of this point, that we are unable to follow Professor Cason very far in the application of such a criticism to the Britt or Kellogg statements (**4**, **5**). It seems to us, in fact, as if the very objection which he raises is far more effective against his own definition than it is against any we have proposed. Does the 'establishment or strengthening of neural connections' describe, explain, or define such events as the rat's learning of the maze, the dog's becoming conditioned, the child's learning to speak, or the graduate student's mastery of psychology? The plain facts of the matter are that in all of these instances the reactions of the organism to the same stimulus situation are different after awhile from what they were at first. It is hard for us to conceive how such phenomena, which are so clearly and directly activity or behavior, can possibly be defined in terms of neurological connections.

The rôle of the nervous system.—The really important issue has to do with the function of the nervous system in learning, and whether the probable changes in nerve structure are well enough established to be included in the definition of learning.[3] Britt has asked with reference to this question, "Since the 'establishment or strengthening of neural connections' during learning is hypothetical, why introduce neural changes at all" (1, p. 463)? Kellogg's objections to speaking of learning as the establishment or strengthening of neural connections have been given at some length elsewhere (15). In brief, they are as follows. First, the Cason definition overemphasizes a physiological inference at the expense of the more fundamental and directly observable facts of behavior. Second, the actual use or application of this definition would mean that we could never tell whether an individual had learned without examining his nerve tissues. Third, the structural definition denies the possibility of learning to lower organisms which do not possess nervous systems, and so cannot have 'neural connections'—yet they seem to be capable of learning.

Some current experiments.—It may be argued that a criticism dealing with lower organisms is limited and highly specialized. It hardly applies to the field which a structural definition of learning is intended to cover. We should like, therefore, to emphasize here the extreme fallibility of present-day knowledge with reference to the part played by the nervous system in the learning of *higher* organisms. Some experiments on conditioning in dogs will be cited to illustrate this fact. Loucks (17, 18, 19) has shown that the peripheral receptor and the *afferent nerve pathway* of the conditioned stimulus are not necessary in order to train dogs to lift the paw to a conditioned signal. The signal can be introduced midway in the reflex arc by stimulating the cortex directly,

[3] The same issue exists with regard to the concept of forgetting, concerning which McGeoch has said, "The frequently made statement that forgetting is a matter of increased synaptic resistance, or of impaired connections in a network of neural arcs, is interesting speculation, but it does not advance our understanding of the problem" (21, p. 368).

and still the animal will learn to flex its leg in response to this signal.

Light and Gantt (16) have further demonstrated that the peripheral portions of *efferent nerve pathways* and the effector organ itself are not necessary to learning which involves that organ and its motor nerves. This striking result was accomplished by pinching off the motor roots of seven of the spinal nerves in such a way that rapid regeneration took place. Since the sensory nerves were left intact, electric-shock stimuli could be applied to the paralyzed limb without difficulty. Animals were then trained by the buzz-shock technique *with the shock administered to the paralyzed member.* All of the training was undertaken while the limb was completely flaccid. After regeneration had taken place and the normal function had returned to the previously paralyzed limb, retention tests, using the buzz alone, were given. Some of the animals in this situation demonstrated that they had learned to lift the foot without movement, that is, *without the functioning of the peripheral motor pathways at all.* By combining his own results from this and from some other experiments with those of Loucks (20), Gantt has recently concluded that the central nervous system is the organ of learning par excellence, and that peripheral nerve structure is only of minor or secondary consequence (11, 12). Such a conclusion is one which would no doubt gratify Professor Cason.

But there are other experiments which make a conclusion of this sort appear at the present time to be unjustified and, in fact, extremely questionable. (*a*) The first of these are the striking demonstrations of Professor Culler and his colleagues who have shown that 'generalized' conditioning is possible in dogs when the highest center of the central nervous system, namely, the cerebral cortex, is entirely missing. This phenomenon has been successfully studied in two specimens with confirmatory results (9, 13). (And incidentally, it has also been demonstrated by other investigators (14) in animals beside the dog.) (*b*) The second of these are the further observations of Culler (7, 8) to the effect that *something*

which looks like simple conditioning is even possible in the spinal animal. After the cord has been transected near the third cervical vertebra, a shock is applied to the hind forepaw. This causes an observable contraction in the exposed semi-tendinosus muscle. The shock-to-the-paw is then regularly preceded by a milder shock-to-the-tail which normally elicits no reaction in the leg muscles. Eventually behavior very similar to conditioning takes place so that the semi-tendinosus muscle contracts to the conditioned stimulus of shock-to-the-tail.

Culler himself is extremely cautious about the interpretation of this remarkable behavior. It may be real learning. It may be something else. He has written regarding it ". . . we do not commit ourselves on the true nature of the phenomenon. It has certain obvious analogies to conditioning, but we are not yet certain that it is like conditioning in all respects. . . . The phenomenon, although repeatedly confirmed, seems to be somewhat erratic in certain cases."[4] Whatever its true nature, the findings at the present time throw grave doubt upon the function of the higher nervous system in learning.

If now we put the striking observations of Culler together with those of Loucks and Gantt, we may be led to conclude, in our more optimistic moments, that perhaps an organism can learn simple acts of conditioning without any of the physiological structures which were formerly supposed to be indispensable. For it would appear as though receptors, effectors, both sensory and motor peripheral nerves, the cerebral cortex, and in fact the entire brain and the upper part of the spinal cord *may* be unnecessary for learning to take place. What is left except the spinal cord below the cervical region? Is it here that Professor Cason's connections are strengthened?

Scientific conservatism would, of course, prevent a serious conclusion like that just suggested. It would be going as far in one direction as the structural definition goes in the other. But it should make clear how little is really known of the

[4] Personal communication to one of the writers.

operation of the nervous system in learning. And so it should invalidate a definition of learning based upon neural modifications.

Is an account like that which has been given biased and unfair? Is there direct evidence for neural changes in learning which has been covered up or overlooked by the present writers? We have elsewhere cited Freeman with regard to this question. He writes as follows: "Practically nothing is known of the structuro-functional changes in nerve tissue which accompany learning" (**10**, p. 488). Without going into detail on the evidence that exists, which would unnecessarily lengthen this discussion, we may refer to the recent treatise of Chappell and Pike (**6**) who incline to the view that the neural changes in learning are probably an increase in the size and mass of the neurones similar to the increase in the size of muscle cells with use. This is the only tenable hypothesis, say these authorities, since (*a*) objections against reduction in synaptic resistance are 'fatal' and (*b*) any other view implies increased capacity for work without an increase in mass, thereby "postulating perpetual motion which is of doubtful occurrence in living organisms" (p. 289).

Chappell and Pike bring together a number of experiments bearing on the question at issue, yet their best data for neural changes during learning are inferential and indirect. The principal arguments are in line with the following: (*a*) Nerves degenerate with disuse and hence they must grow with use. (*b*) Young nerve cells fail to develop properly if not used. Following is a portion of their own appraisal of the situation: "The internal processes, increased metabolism and a resulting increase in the size of the nerve fiber have not yet been observed during learning. But since the same general characteristics are found in most tissues, there exists a possibility, and by the method of inductive reasoning even a high probability, that similar changes occur in muscle and nerve under similar conditions" (**6**, p. 288).

The probability of structural nerve changes of some sort has been clearly stressed throughout the present paper. But it has been stressed as *a probability*, and not as a certainly known fact.

Observations or implications?—It should be obvious from this accumulation of evidence that there are at present no directly observable data behind what Professor Cason is pleased to refer to as 'symptoms.' The symptoms themselves constitute the only real facts we have. True, our knowledge on this question is in a state of flux and development, so that it may soon be demonstrated beyond any doubt that changes do take place in the nervous system of higher organisms during learning. But since such changes remain now in the inferential stage, they should be discussed as inferential and not described as factual.

The scientific worker, to our way of thinking, should know exactly where his facts end, and his inferences, hypotheses, and assumptions begin. Experimental and other observations are continually enlarging and broadening the factual material with which we have to deal. Yet there is always a boundary where observable knowledge leads into the unknown. If we apply the law of parsimony to definitions as we do to observations and interpretations, we should see at once that there is no more justification in 'jumping to conclusions' in a definition than there is in any other statement.

A scientific definition, after all, is no more than a brief summary of the scientific data which lie behind it. It may be general or specific, broad or narrow, and it possesses of necessity the characteristic imprint of its author. But it should be based—as is science itself—upon provable, observable fact. To deviate from this principle, and to base so elementary and fundamental a statement as a definition upon assumed neurological changes makes the definition not a statement of fact, but a hypothetical inference. It then becomes an anomalous implication, which does not define at all, in a precise scientific sense.

V. Summary

This article is an attempt to clarify the concept of learning by comparing the merits and demerits of two distinct points of view recently taken by Cason on the one hand and by the present writers on the other hand. The Cason viewpoint

would define learning in terms of structure (neurological connections) while the Britt-Kellogg view would define learning in terms of function (behavioral changes). The principal criticism of the functional position has been that it considers only the 'symptoms' (behavior) of the true learning which is inside the organism. The structural position, as typified by Cason's definition, has been found unsatisfactory because,

1. It really does not define or include any of the commonplace instances of learning.

2. It bases learning upon 'the establishment or strengthening of neural connections,' a structural change which probably does take place, but which remains at the present time so obscure and unobservable that it must be regarded as inferential.

3. Recent experiments in neuropsychology have shown that in animals as high in the phyletic scale as the dog, simple learning can be demonstrated in preparations without effectors, without conditioned receptors, without peripheral motor and sensory nerves, without the cerebral cortex, and possibly even without the entire brain and the upper part of the spinal cord. Such findings demonstrate the extreme fallibility of our knowledge of the operation of the nervous system during learning. There may be no such thing as 'the establishment or strengthening of neural connections.'

The functional concept of learning is therefore the only safe and conservative one, considering the present state of our knowledge. It follows the law of parsimony and does not go beyond observable data.

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